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Oliver Wendell Holmes and the Contagiousness of Puerperal Fever.*

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Turning over in my mind how I could best utilize this occasion, it appeared to me that here at last had come my chance to fulfil an intention I had long cherished of reminding my professional brethren of the debt we owe to Oliver Wendell Holmes for his powerful, but I am afraid largely forgotten, essay on the contagiousness of puerperal fever.

No one who is acquainted with my teaching or writings, or who remembers the active part I took some ten or twelve years ago as one of the Secretaries of the Semmelweis Memorial Fund in this country, will suspect me of failing to recognize the great work, accomplished "with tears and travail," of that illustrious man.† It is possible, however, to do justice to Semmelweis and yet to be ready to acknowledge the work done by others in a similar direction.

I purpose to-day to call attention afresh to Holmes's memorable

* An address delivered to the Trowbridge Division of the Bath and Bristol Branch of the British Medical Association.

† The credit of having been the first to make known in Great Britain the work that Semmelweis was doing in Vienna belongs to Dr. C. H. F. Routh, who, on his return in 1848 from a visit to that city, read a paper on the subject before the Royal Medical and Chirurgical Society of London (see *Med. Chir. Trans.*, 1849). In 1886 Dr. Theodore Duka, a fellow countryman of Semmelweis, contributed to the *Lancet* a most interesting and sympathetic account of his life and labours, founded on the more elaborate work of Bruck, which had then recently been published. Dr. Duka's paper was afterwards reissued in the form of a pamphlet.

essay, and to try to do something towards removing the reproach—not, I fear, entirely undeserved—that we English-speaking people on both sides the Atlantic,* whilst giving abundant honour to Semmelweis, have been in danger of forgetting the earlier† and equally remarkable contribution to our knowledge of puerperal fever that we owe to one of our own kith and kin. This forgetfulness has been due, not so much, perhaps, to the overshadowing of Holmes's work by that of Semmelweis, as to the fact that the medical reputation of Holmes himself was put into the shade by the brilliant success he achieved in later years in general literature, and that his own profession came to be proud of him as the Autocrat, Professor, and Poet of the breakfast-table, rather than as the fearless and outspoken defender of the life and health of the parturient woman. Holmes has himself left on record an account of the origin of the essay of which I am about to speak. At a meeting of the Boston (U.S.A.) Society for Medical Improvement, of which Wendell Holmes was a member, the stated business having been concluded somewhat early, one of the members, in order to fill up the time, reported an incident that had recently occurred in the neighbourhood. A physician had made a *post-mortem* examination of the body of a puerperal fever patient, and had himself died in less than a week apparently in consequence of a wound received at the examination. In the meantime, he had attended several women in confinement, all of whom were said to have been attacked with puerperal fever. The discussion that followed the relation of this case made it clear to Holmes that "a fuller knowledge of the facts relating to the subject" was much needed, and he therefore felt that it would be doing a good service "to learn what experience had to teach in the matter." . . . He embodied the results of his inquiry in an essay which he read before the Society and which, at the Society's request, he subsequently published in the *New England Quarterly Journal of Medicine and Surgery* for April, 1843. As this Journal had only a very restricted circulation and died a natural death when it was but a year old, the essay was practically

* Two of the latest and most important American text-books of obstetric medicine do not even mention him.

† Holmes's essay was published in April, 1843, a year before Semmelweis graduated in Medicine and three years before he made his earliest observations on puerperal fever. The results of Semmelweis's observations were first published by Hebra in 1847-8. He himself did not publish anything on the subject until 1858. His great work "Die Ætiologie, etc.," appeared in 1861. I know of no evidence to show that either Holmes or Semmelweis mentioned each other's work or indeed knew of it.

buried, and, until its reissue with additions in 1855,* cannot be said to have been brought fully before the profession.

Let me try to give an epitome of the evidence contained in this essay and of the arguments founded upon it, and I will do so as far as possible in the author's own words. The essay commences, then, with the statement that the present inquiry has not been undertaken because of any doubt in the minds of well-informed members of the medical profession "as to the fact that puerperal fever is sometimes communicated from one person to another, both directly and indirectly." Such doubts the author would consider "merely as a proof that the sceptic had either not examined the evidence, or, having examined it, had refused to accept its plain and unavoidable consequences." "It signifies nothing," he says, "that wise and experienced practitioners have sometimes doubted the reality of the danger in question; no man has the right to doubt it any longer. No negative facts, no opposing opinions, be they what they may or whose they may, can form any answer to the series of cases now within the reach of all who choose to explore the records of medical science." In regard to the relevancy of negative facts he reminds his readers that "children that walk in calico before open fires are not always burnt to death." The instances to the contrary may indeed, like other truths, be "worth recording"; but not "if they are to be used as arguments against woollen frocks and high fenders."

There is another criticism that he foresees and forestalls. "It may be said that the facts are too generally known and acknowledged to require any formal argument or exposition." This objection he sweeps aside by a reference to two of the leading American obstetric treatises of the time. In one of these, *Dewees on the Diseases of Females*, a book at that time of very wide repute throughout the United States, it was expressly stated, in the very latest edition, that so far as that country was concerned, puerperal fever had not hitherto appeared under any circumstances that afforded "the slightest ground for the belief that it is contagious." In the other, *The Philadelphia Practice of Midwifery*, a work of scarcely less authority than Dewees, not one word could be found "in the chapter devoted to this disease, which would lead the reader to suspect that the idea of contagion had ever been entertained."

* The title of the original essay was "The contagiousness of puerperal fever." The 1855 reprint was entitled "Puerperal fever as a private pestilence." The essay will also be found included in a volume published at Boston, U.S.A., in 1861, under the title of "Currents and counter-currents in medical science," and again in "Medical Essays, 1842—82," Boston, U.S.A., 1883, as well as in the collected works. An abstract of the original paper appeared in the *Amer. Journ. Med. Sciences*, July, 1843, pp. 260—264.

Having thus shown the need for the inquiry, he proceeds to formulate the thesis which, at its close, he finds himself in a position to defend. This thesis he presents in the following words: "*The disease known as puerperal fever is so far contagious as to be frequently carried from patient to patient by physicians and nurses.*"

Before submitting direct evidence in support of this statement he rids himself of certain *impedimenta* in the shape of side issues. For example, he at once admits the possibility that what is called puerperal fever may not be equally contagious in all its forms. He refuses to "enter into any dispute about the particular *mode* of infection, whether it be by the atmosphere the physician carries about him into the sick-chamber, or by the direct application of the virus to the absorbing surfaces with which his hand comes in contact."

He points out that it is not necessary in order to prove his thesis that "the contagion of puerperal fever must always be followed by the disease," it being "true of all contagious diseases that they frequently spare those who appear to be fully submitted to their influence." He grants, more readily perhaps than we should do nowadays, "that the disease may be produced and variously modified by many causes besides contagion, and more especially by epidemic and endemic influences."

And having thus cleared the air, he marshals his witnesses and proceeds to build up a mass of evidence, which, though its cogency was doubted at the time, as we shall presently see, has now long been acknowledged to be far beyond the reach of refutation. Let us very briefly pass this evidence in review. Some of the witnesses are familiar to us. Of these, most, if not all, are our own countrymen. But there are also witnesses—mostly American—whose evidence, though equally striking, has not found its way into our treatises and text-books, and has not, therefore, in the same sense become common property. We will take first, as the writer himself does, certain British witnesses, beginning with that fine old eighteenth century surgeon, Charles White of Manchester.

"I am acquainted," says White (1773), "with two gentlemen in another town, where the whole business of midwifery is divided betwixt them, and it is very remarkable that one of them loses several patients every year of the puerperal fever, and the other never so much as meets with the disorder." That the full significance of this circumstance was not perceived at the time even by White himself does not diminish its value as a piece of evidence. Then comes the

more personal testimony of Dr. Gordon of Aberdeen,* published in 1795, which, though it has been quoted over and over again, is still worth repeating, not only because of its early date, but also because, as Holmes points out, Gordon's "expressions are so clear," and "his experience is given with such manly distinctness and disinterested honesty." "This disease," he says, "seized such women only as were visited or delivered by a practitioner, or taken care of by a nurse, who had previously attended patients affected with the disease." "I had evident proofs of its infectious nature and that the infection was as readily communicated as that of the small-pox or measles, and operated more speedily than any other infection with which I am acquainted." "I had evident proofs that every person who had been with a patient in the puerperal fever became charged with an atmosphere of infection which was communicated to every pregnant woman who happened to come within its sphere." "It is a disagreeable declaration," he adds, "for me to mention that I myself was the means of carrying the infection to a great number of women." He cites a number of instances in which the disease was conveyed by midwives and others, and says: "These facts fully prove that the cause of the puerperal fever . . . . was a specific contagion, or infection, altogether unconnected with a noxious condition of the atmosphere." But the most startling of his statements is still to come. "I arrived," he says, "at that certainty in the matter, that I could venture to foretell what women would be affected with the disease upon hearing by what midwife they were to be delivered, or by what nurse they were to be attended, during their lying-in; and almost in every instance my prediction was verified," a piece of evidence so terrible that Holmes printed it in capital letters.

The next witness summoned is Dr. Armstrong, of Sunderland, the author of a well-known essay on puerperal fever, in which he describes 43 cases as having occurred in Sunderland between January 1st and October 1st, 1813, of which number 40 were the patients of a single practitioner, and were all attended by himself and his assistant, whilst the remaining three were distributed amongst three other

* Dr. W. Stephenson, Professor of Midwifery in the University of Aberdeen, has placed in his class-room an oak tablet with the following inscription: "Alexander Gordon, M.D., first demonstrated the infectious nature of puerperal fever, Aberdeen, 1795." A short notice of Gordon, by his grandson, a former Professor of Materia Medica in Aberdeen, will be found in one of the volumes of the (old) Sydenham Society, entitled *Essays on the Puerperal Fever and other Diseases Peculiar to Women; Selected from the Writings of British Authors Previous to the Close of the Eighteenth Century.* Edited by Fleetwood Churchill. London, 1849. This volume also contains a reprint of Gordon's pamphlet.

doctors. The practitioner, Mr. Gregson, who had the forty cases, had no hesitation in asserting that the disease, as it appeared in his practice, was highly contagious, and was communicable from one puerperal woman to another. Like Dr. Gordon, he scorned to avail himself of any sheltering theory about epidemics or drains, and boldly and manfully makes his confession. "However painful to my feelings," he writes, "I must in candour declare that it is very probable the contagion was conveyed in some instances by myself, though I took every possible care to prevent such a thing from happening the moment that I ascertained the distemper was infectious."

It would be tedious to give in detail the testimony of all the British witnesses much as I should like to do so. Let it suffice to say that statements of a precisely similar character to those already given are quoted from Davies, Gooch, Ramsbotham, Roberton, Blundell, Hutchinson, King, and Lee. The account by the second William Hey, of Leeds, of an outbreak which occurred in that town and its vicinity in the years 1809–12, appears to have escaped notice.

"The recurrence," he says, "of long series of cases like those I have cited, reported by those most interested to disbelieve in contagion, scattered along through an interval of half a century might have been thought sufficient to satisfy the minds of all inquirers that here was something more than a singular coincidence. But if, on a more extended observation, it should be found that the same ominous groups of cases clustering about individual practitioners were observed in a remote country, at different times, and in widely separated regions, it would seem incredible that any should be found too prejudiced or indolent to accept the solemn truth knelled into their ears by the funeral bells from both sides of the ocean, the plain conclusion that the physician and the disease entered, hand in hand, into the chamber of the unsuspecting patient."

That such series of cases *had* been observed in the United States and even in his own immediate neighbourhood, he then proceeds to show. "Certainly nothing," he says, "can be more open and explicit than the account given by Dr. Peirson, of Salem, of the cases seen by him. In the first nineteen days of January, 1829, he had five consecutive cases of puerperal fever, every patient he attended being attacked and the first three cases proving fatal. In March of the same year he had two moderate cases, in June another case, and in July another, which [last] proved fatal. 'Up to this period,' he remarks, 'I am not informed that a single case occurred in the practice of any other physician.'" It appears that Dr. Peirson had

altogether in his practice 20 cases of puerperal fever, of which 4 were fatal.

In the year 1842 the attention of the College of Physicians of Philadelphia was called to the prevalence of puerperal fever of a peculiarly insidious and malignant character in the practice of one of the Fellows of the College. Every woman that this physician had attended during several weeks past was said to have been attacked by the fever, while no instance of the disease had occurred amongst the patients of any other accoucheur practising within the same district.

Dr. Condie, who reported the circumstances, although disposed to be sceptical as to the contagious nature of many so-called contagious diseases, declared himself convinced by the facts that had come under his notice that the puerperal fever in the particular form it had on this occasion assumed was certainly communicable. The physician referred to (Dr. Rutter) stated that a succession of cases having occurred in his practice he went away for a week, but that he could not readily believe in the transmissibility of the disease from patient to patient or from physician to patient, inasmuch as one of the very first patients he attended after his return was attacked by the fever and died, notwithstanding that he had not used on that occasion any article of clothing he had used before.

These remarks were made on May 3rd, 1842, and in a letter dated December 20th in the same year, Dr. Rutter speaks of having had a personal experience of nearly seventy of these "horrible cases," all of them within the past twelve months.* The obstetric physician to whom this letter was addressed naïvely suggests that this experience of Dr. Rutter's was in all probability to be explained by the midwifery practice of the district being largely in his hands.

At another meeting of the same college "Dr. Warrington stated that a few days after assisting at an *autopsy* in a case of puerperal peritonitis in which he ladled out the contents of the abdominal cavity with his hands, he was called upon to deliver three women in rapid succession." All were attacked with puerperal fever. Soon afterwards "he saw two other patients, both on the same day, with the same disease. Of these five patients two died."

At the same meeting Dr. West related that Dr. Samuel Jackson, whilst practising in Northumberland County (U.S.A.) attended, in rapid succession, seven women who were all attacked with puerperal fever and of whom five died. "Women," said Dr. Jackson, "who had expected me to attend upon them, now becoming alarmed, re-

* *Medical Examiner*, Philadelphia, January 21st, 1843.

moved out of my reach, whilst others sent for a physician residing several miles off. These women, as well as those attended by midwives, all did well." Only two other cases of death in childbed were known to have occurred during the same period within a radius of 50 miles, and both these deaths were afterwards ascertained to have been caused by other diseases. Dr. Jackson "underwent, as he thought, a thorough purification, and still his next patient was attacked with the disease and died. He was led to suspect that the contagion might have been carried in the gloves which he had worn in attendance upon the previous cases. Two months or more after this he had two other cases. He could find nothing to account for these unless it was the instruments for giving enemata, which had been used in one or two former cases and were employed by these patients. When the first case occurred he was attending and dressing a limb extensively mortified from erysipelas, and he went immediately to the accouchement with his clothes and gloves most thoroughly imbued with its effluvia."

Up to this point the evidence adduced has been gathered entirely from published records. The next three items of evidence were now made public for the first time. All the cases referred to occurred in the State of Massachusetts, and two of the three series "in Boston and its immediate vicinity."

The history of the first of these three series is as follows:—

On March 19th, 1842, a physician, Dr. C., made a *post-mortem* examination in the case of a man who had died after an illness of only forty-eight hours "with œdema of the thigh and gangrene extending from a little above the ankle into the cavity of the abdomen." Whilst conducting the autopsy Dr. C. wounded himself slightly in the right hand. During the night immediately following he attended a patient in labour, the wounded hand being at the time very painful. The woman died of puerperal fever on the 24th. Dr. C. was unable to visit her after the confinement, being himself ill from the wound in his hand and unable to leave the house until April 3rd. On April 9th he delivered another patient, who died of childbed fever on the 14th. On April 10th and 11th he delivered two other patients, both of whom died, one on April 14th and the other on the 8th. On April 27th he attended a fifth patient, who died on May 3rd. On April 18th he delivered another woman who developed symptoms of puerperal fever but recovered. He now left town for a few days, but on May 8th he again attended a confinement. This patient, the seventh of the series, became ill, but recovered. About July 1st this same doctor was called to deliver a patient in a neighbouring village. The woman died in two or three days. No other cases occurred at the time in the practice of any of the physicians in the town or vicinity. The nurse who laid out the body of the third patient was seized the same evening with sore throat and erysipelas from which she died in ten days. The nurse who had laid out the body of the

fourth patient was taken ill the following day, and died in a week. Several cases of erysipelas occurred in the house, where the autopsy mentioned above took place, soon after the examination. It may be mentioned as a remarkable circumstance that Dr. C.'s partner, who assisted at the autopsy and was in attendance upon all the cases of erysipelas that followed it, had 12 midwifery cases between March 26th and April 12th, the patients all making a normal recovery.

The second series of cases that Holmes now for the first time placed on record occurred in the year 1830.

A doctor attended a patient on February 4th, and she died on the 12th. During the following month the same physician delivered eight women, all of whom did well with one exception. This patient was confined on February 28th and died on March 8th. The doctor inspected the body on the 9th, and the night after he attended a lady, who became ill, and died on the 16th. On the 10th he attended another patient, who sickened but recovered. On the 16th he went direct from this patient's room to another confinement. The lady became ill and died on the 21st. On the 17th he made an autopsy in one of the fatal cases. On the 19th he delivered another lady, who sickened, and died on the 22nd. This made 6 cases with only one recovery. He now refused to attend any labour until April 21st, when, having thoroughly cleansed himself, he resumed his practice and had no more puerperal fever.

He had had some previous experience of puerperal fever in his practice, but until now he had not entertained any suspicion that the disease could be communicated. When a few years afterwards he met with another series of cases, he tells his friend, Dr. Storer, that he was so fully convinced of the communicability of the disease, that whilst in attendance on the cases he changed his clothes, and washed his hands in a solution of chlorinated lime* after each visit.

The third series of original cases mentioned in Holmes's essay occurred in his own city of Boston in the summer of 1842. A physician lost five cases in succession from puerperal fever between May 7th and June 17th. For two weeks previous to his first case he had been attending a severe case of erysipelas.

"This long catalogue of melancholy histories," says Holmes, "assumes a still darker aspect when we remember how kindly Nature deals with the parturient female when she is not immersed in the virulent atmosphere of an impure lying-in hospital, or poisoned in her chamber by the unsuspected breath of contagion."

He next goes on to "mention a few instances in which the disease appears to have been conveyed by the process of direct inoculation." He refers, for example, to the well-known story of Dr. Campbell of Edinburgh, who, in October, 1821, assisted at a *post-mortem* examination in a case of puerperal fever, carrying the pelvic viscera in his

* It will be remembered that solution of chlorinated lime was the disinfectant used by Semmelweis.

pocket to the class-room. The same evening he attended a woman in labour, and he was called to a second the following morning. Both these patients died, as well as three others whom he delivered within the next few weeks. Many others of his patients were seized with puerperal fever, and though they escaped with their lives, they did so only after a long illness.

This was not Dr. Campbell's only experience of the kind. "In June, 1823, he assisted some of his pupils at the *autopsy* of a case of puerperal fever. He was unable to wash his hands with proper care, for want of the necessary accommodation." On reaching home he found that two midwifery patients had sent for him. He went to their assistance without washing his hands or changing his clothes; "both these patients died with puerperal fever."

Further instances of a similar character are quoted from Roberton, Ingleby, Rigby, Merriman and others.

In an unsigned article in the *British and Foreign Medical Review* for January, 1842, said to have been from the pen of no less an authority than Dr. Rigby, there is narrated the following case:—

> A young practitioner, contrary to advice, examined the body of a patient who had died from puerperal fever; there was no epidemic at the time; the case appeared to be purely sporadic. He delivered three other women shortly afterwards; they all died with puerperal fever, the symptoms of which broke out very soon after labour. He assisted to remove some coagula from the uterus of one of the patients of his colleague, and this woman was attacked in the same manner and died, whilst all the rest of his colleague's patients did well.

The case quoted from Dr. Merriman was related by him at a meeting of the Royal Medical and Chirurgical Society of London, and is worth briefly reproducing as it illustrates the risks an obstetrician runs of infecting his patients even if he is merely present at the *post-mortem* examination in a case of puerperal fever without taking any part in the manipulations. "Dr. Merriman was at the examination of a case of puerperal fever at two o'clock in the afternoon. *He took care not to touch the body.* At nine o'clock the same evening he attended a woman in labour; she was so nearly delivered that he had scarcely anything to do. The next morning she had severe rigors, and in forty-eight hours she was a corpse. Her infant had erysipelas and died in two days."

". . . add to all this," says Holmes, "the undisputed fact that within the walls of lying-in hospitals there is often generated a miasm, palpable as the chlorine used to destroy it, tenacious so as in some cases almost to defy extirpation, deadly in some institutions as

the plague, which has killed women in a private hospital in London so fast that they were buried two in one coffin to conceal its horrors, . . . which has led Dr. Lee to express his deliberate conviction that the loss of life occasioned by these institutions completely defeats the objects of their founders; and out of this train of cumulative evidence, the multiplied groups of cases clustering about individuals, the deadly results of *autopsies*, the inoculation by fluids from the living patient, the murderous poison of hospitals—does there not result a conclusion that laughs all sophistry to scorn and renders all argument an insult?"

"It is true," he goes on to say, "that some of the historians of the disease . . . profess not to have found puerperal fever contagious. At the most, they give us mere negative facts, worthless against an extent of evidence which now overlaps the widest range of doubt, and doubles upon itself in the redundancy of superfluous demonstration."

The President of one of the principal American life insurance companies, on being asked his views as to insuring the life of the next patient of a doctor who had had a succession of ten, five, three, even two fatal cases in his practice, naturally replied that he would require a very large extra premium, if indeed he consented to take the risk at all. His examination of the recorded facts called forth just such expressions of indignation as might be expected, and just such as would soon have swelled into a general cry of horror had the hideous catalogue of cases, as Wendell Holmes says, ever been fully brought to the knowledge of the public.

It is not to be wondered at that such an *exposé* should rouse the active opposition of those members of the medical profession who were still unconvinced as to the contagiousness of puerperal fever. We owe, indeed, to the fact that such opposition did arise, and from very influential quarters, the republication of the essay in 1855, twelve years after its first appearance. Two widely-known professors in two of the largest medical schools of the United States had recently expressed their disbelief of the doctrine that it was the object of Holmes's essay to enforce. One of these was Professor Hodge, who held the Chair of Obstetrics in the University of Pennsylvania. The other was Dr. Meigs, Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College, Philadelphia.*

* An excellent and impartial sketch of the life and work of both these professors will be found in Whitridge Williams's sketch of the *History of Obstetrics in the United States up to 1860*, pp. 38 to 46. (Published originally in Professor Dohrn's *Geschichte der Geburtshülfe der Neuzeit, zugleich als dritter Band des Versuchs einer Geschichte der Geburtshülfe*, von Eduard von Siebold, Tübingen, 1903. Erstè Abtheilung, pp. 193—264.)

The former of these, in an introductory lecture "On the Non-contagious Character of Puerperal Fever" delivered at Philadelphia on October 11th, 1852, summed up as follows:—

> The result of the whole discussion will, I trust, serve not only to exalt your views of the value and dignity of our profession, but to divest your minds of the overpowering dread that you can ever become —especially to woman, under the extremely interesting circumstances of gestation and parturition—the minister of evil; that you can ever convey, in any possible manner, a horrible virus so destructive in its effects and so mysterious in its operations as that attributed to puerperal fever.

Thus, with fair words he waved, as it were, into space what he did not wish to believe; but his lecture was, at any rate, couched in unobjectionable and inoffensive language. Not so was the chapter in Dr. Meigs's volume which treated of *Contagion in Childbed Fever*. There are in it expressions which, as Wendell Holmes said, "might well put a stop to all scientific discussions were they to form the current coin in our exchange of opinions." But Holmes declined to take offence or attempt any retort. "No man," he says, "makes a quarrel with me over the counterpane that covers a mother with her newborn infant at her breast. There is no epithet in the vocabulary of slight and sarcasm that can reach my personal sensibilities in such a controversy." And so he contented himself, in an introduction to the reprinted essay, with a detailed examination of the various points raised by Dr. Meigs, and with some remarks on the comparative worthlessness of negative evidence,* to which Dr. Meigs attaches undue importance. Dr. Meigs's position may be illustrated by a single quotation. Speaking of such outbreaks of puerperal fever as those described by Wendell Holmes, he says: "I prefer to attribute them to accident, or Providence, of which I can form a conception, rather than to a contagion of which I cannot form any clear idea, at least as to this particular malady." A practitioner who meets with epidemic cases is for him simply "unlucky." "We do not deny," says Holmes, "that the God of battles decides the fate of nations; but we like to have the biggest squadrons on our side, and we are

* That is, exposure without subsequent disease. On this point Holmes quotes the following passage from the chapter on continued fever in Watson's *Lectures on the Practice of Physic*. "A man might say, 'I was in the battle of Waterloo and saw many men around me fall down and die, and it was said that they were struck down by musket-balls; but I know better than that, for I was there all the time, and so were many of my friends, and we were never hit by any musket-balls. Musket-balls, therefore, could not have been the cause of the deaths we witnessed.'" He also refers to the statement of John Hunter that he knew a case in which of twenty-one persons bitten by a rabid dog, only one died of hydrophobia.

particular that our soldiers should not only say their prayers but keep their powder dry."

"The subject of my paper," he writes in 1855, "has the same profound interest for me at the present moment as it had when I was first collecting the terrible evidence out of which, as it seems to me, the commonest exercise of reason could not help shaping the truth it involved. It is not merely on account of the bearing of the question—if there is a question—on all that is most sacred in human life and happiness, that the subject cannot lose its interest. It is because it seems evident that a fair statement of the facts must produce its proper influence on a very large proportion of well-constituted and unprejudiced minds. Individuals may, here and there, resist the practical bearing of the evidence on their own feelings or interests; some may fail to see its meaning, as some persons may be found who cannot tell red from green; but I cannot doubt that most readers will be satisfied and convinced, to loathing, long before they have finished the dark obituary calendar laid before them. I do not know," he continues, "that I shall ever again have so good an opportunity of being useful as was granted me by the raising of the question which produced this essay. For I have abundant evidence that it has made many practitioners more cautious . . . and I have no doubt it will do so still, if it has a chance of being read, though it should call out a hundred counterblasts. . . ." "I do not expect," he says, in another place, "ever to return to this subject. . . . I trust that I have made the issue perfectly distinct and intelligible; . . . this is no subject to be smoothed over by nicely-adjusted phrases of half assent and half censure divided between the parties. The balance must be struck boldly and the result declared plainly. If I have been hasty, presumptuous, ill-informed, illogical; if my array of facts means nothing; if there is no reason for any caution in view of these facts; let me be told so on such authority that I must believe it, and I will be silent henceforth, recognising that my mind is in a state of disorganization. If the doctrine I have maintained is a mournful truth; if to disbelieve it, and to practise on this disbelief, and to teach others so to disbelieve and practise, is to carry a desolation, and to charter others to carry it, into confiding families, let it be proclaimed as plainly what is to be thought of the teachings of those who sneer at the alleged dangers and scout the very idea of precaution. Let it be remembered that *persons* are nothing in this matter; better that twenty pamphleteers should be silenced, or as many professors unseated, than that one mother's life should be taken. There is no quarrel here between men, but

there is deadly incompatibility and exterminating warfare between doctrines. . . . Let the men who mould opinions look to it; if there is any voluntary blindness, any interested oversight, any culpable negligence even, in such a matter, and the facts reach the public ear, the pestilence carrier of the lying-in chamber must look to God for pardon for man will never forgive him."

And thus, with burning words forged at that white heat which alone befitted the subject or could give adequate expression to the writer's intense convictions, he brings his introduction to a close. When one reads these words now—I hope I may say it without irreverence—it is impossible to avoid comparing them with the eloquent but terrible warnings of the preacher-prophets of the Hebrew scriptures. It was not the fault of these old stalwarts that the message they had to deliver was unillumined by a gospel that had not yet been revealed, any more than it was the fault of Wendell Holmes and of the distinguished Hungarian who, a few years later, and as the result of independent observation, arrived at the same truth, and encountered an opposition even more relentless, that they were unable to point to the more excellent way of prevention with which we, thanks to later revelations, have since become familiar. Pasteur and Lister had not as yet instituted those researches of which the result has been to place in our hands, if we would but use it, an effectual means not only of checking the spread of puerperal fever but of stamping it out. All honour to the men who, without these researches to guide them, and merely from a study of clinical facts, recognised the main factors in the propagation of puerperal fever, and advocated for its prevention measures which differ in degree rather in kind from those enjoined by the most scientific and advanced teachers of the present day.

It is interesting to remember, in connection with recent discussions as to the time of life at which a man turns out his best work, that this essay was published when Holmes was 33. It was not until four years later that he was appointed to the Parkman Professorship of Anatomy and Physiology in the medical school of Harvard University, which he described as being, "not so much a chair as a whole settee," and the anatomical portion of which he retained for thirty-six years. He had as yet no literary fame except such as was derived from his contributions to various college magazines, a prize essay on Intermittent Fever in New England (1837), and two lectures on "Homœopathy and its Kindred Delusions," delivered before the Boston Society for the Diffusion of Useful Knowledge in 1842. But if anyone is inclined to quote this early essay as a proof that

a man produces his best work before he is 40, it must be remembered, on the other side, that the earliest chapters of the Breakfast Table series, which first made him famous and upon which his literary reputation really rests, were not published until the year 1857, when their author had reached the comparatively mature age of 48.*

Nevertheless, the fact that a physician comparatively so young and so obscure, and with as yet no official position† to ensure him a hearing, should have dared to publish such an essay as that on puerperal fever, shows him to have been endowed with singular courage and remarkable independence of judgment.

The question was once put to him, in a Canadian journal, by an enthusiastic admirer bearing the honoured name of William Osler, whether he had derived the greater satisfaction from having been the author of that exquisite little poem, *The Chambered Nautilus*, or from having published the *Essay on Puerperal Fever*. The journal reached Wendell Holmes, who thereupon wrote to Professor Osler a letter, the original of which he has, with characteristic kindness, placed in my hands to show to you on this occasion.

The letter bears date January 21st, 1889.

"I have rarely been more pleased," he writes, "than by your allusion to an old paper of mine. There was a time, certainly, in which I would have said that the best page of my record was that in which I had fought my battle for the poor poisoned women. I am reminded of that essay from time to time, but it was published in a periodical which died after one year's life, and therefore escaped the wider notice it would have found if printed in the *American Journal of the Medical Sciences*. A lecturer at one of the great London hospitals referred to it the other day, and coupled it with some fine phrases about myself which made me blush either with modesty or vanity, I forget which.‡

"I think I will not answer the question you put me. I think oftenest of *The Chambered Nautilus*, which is a favourite poem of mine, though I wrote it myself. The essay only comes up at long intervals. The poem repeats itself in my memory, and is very often spoken of by my correspondents in terms of more than ordinary praise. I had a savage pleasure, I confess, in handling those two professors—learned men both

* It is true he had written a few stray chapters in a similar vein twenty-five years previously, but these attracted no attention, and may practically be left out of account.

† He did not become Parkman Professor at Harvard until 1847.

‡ There appears to be good reason for supposing that this allusion has reference to a passage at the close of an address on the preventability of puerperal fever, which the writer delivered at St. Thomas's Hospital in October, 1888.

of them, skilful experts, but babies, as it seemed to me, in their capacity of reasoning and arguing. But in writing the poem I was filled with a better feeling—the highest state of mental exaltation and the most crystalline clairvoyance, as it seemed to me, that had ever been granted to me—I mean that lucid vision of one's thought and all forms of expression which will be at once precise and musical, which is the poet's special gift however large or small in amount or value. There is more selfish pleasure to be had out of the poem, perhaps a nobler satisfaction from the life-saving labour."

In this letter he purposely walks round the question. What was his real feeling, however, towards the essay is shown quite unmistakably in a passage in *The Professor at the Breakfast Table* and from a later letter. The passage in the *Professor* is as follows: "By the permission of Providence, I held up to the professional public the damnable facts connected with the conveyance of poison from one young mother's chamber to another's—for doing which humble office I desire to be thankful that I have lived, though nothing else good should ever come of my life...."

The later letter of which I have spoken has a special interest and pathos, for it is dated 1893, the year before he died, when he was an old man of 83, and could speak of the essay as having been written fifty years ago. Will you bear with me while I read it? It is addressed to his old friend and mine, the late Dr. J. R. Chadwick,† of Boston, the founder and for many years the indefatigable secretary of the American Gynæcological Society:

296, Beacon Street,

May 8th, 1893.

Dear Dr. Chadwick,

You tell me that the President of the Gynæcological Society, Dr. Parvin, proposes to make mention at the meeting of the Society next Tuesday of my early efforts to put a stop to the conveyance of the poison which produces puerperal fever from one lying-in woman to another by the practitioner to whose care she is entrusted.

It is just fifty years since my essay on the contagiousness of puerperal fever was published in the *New England Journal of Medicine and Surgery*. It had been previously read at a meeting of the Boston Society for Medical Improvement, at whose request it was given to the press. The periodical in which it appeared, though well conducted and promising well, died in its first year, having been seen by few readers and supported

† Dr. Chadwick's death took place whilst this address was being written. A sympathetic notice of his life and of the great work he accomplished for the Boston Medical Library was contributed by Professor Osler to the *Lancet* for October 14th, 1905

by few subscribers. My warning and counsel had their effect, however, in our own community, as I have often been assured by competent authorities. The essay attracted respectful notice abroad, as the names of Copland and Ramsbotham and the fifth annual report of the Registrar-General of England sufficiently show. Still, it was not read by some who might have profited by it. If Dr. (afterwards Sir James) Simpson had read the first rule I laid down * he would not have left the record that after freely handling the diseased parts from an autopsy of a victim of puerperal fever his next four childbed patients were affected with that disease.

I thought I had proved my point and set the question of the private pestilence, as I called it, at rest "for good and all." I thought I had laid down rules which promised to ensure the safety of the lying-in woman from disease and death carried to her unconsciously by her professional attendant.

Still, I was attacked in my stronghold by the two leading professors of obstetrics in this country.

I defended my position with new facts and arguments and not without rhetorical fervour at which after cooling down for half a century I might smile if I did not remember how intensely and with what good reason my feelings were kindled into the heated atmosphere of superlatives.

I have been long out of the way of discussing this class of subjects. I do not know what others have done since my efforts; I do know that others had cried out with all their might against the terrible evil before I did and I gave them full credit for it.

But I think I shrieked my warning louder and longer than any of them and I am pleased to remember that I took my ground on the existing evidence before the little army of microbes was marched up to support my position.

Always yours cordially,

OLIVER WENDELL HOLMES.

I have endeavoured in this address to remind my medical brethren of the great service rendered by Oliver Wendell Holmes to science and to humanity by the publication of his remarkable essay. In doing so it may seem to some that I have been needlessly disturbing the ashes of a painful and forgotten controversy, and labouring at a subject that has long ago ceased to have any living interest. But has the subject of the propagation of puerperal fever ceased to have a living interest? Let us look for a moment at the facts. During the fifty-seven years (1847–1903) for which the statistics for England and Wales are available, there were registered

* See Appendix.

no fewer than 93,243 mothers as having died from puerperal septicæmia, and the enormous sacrifice that these figures represent has been going on steadily all the time, and shows no signs of undergoing diminution. If it has changed at all it is in the direction of increase.*

When, in my opening address as President of the Obstetrical Society of London in 1897, I called attention to this state of things, my lamented friend, the late Dr. Milne Murray of Edinburgh read my words, as he afterwards told me, with much surprise, and with the feeling that, however true they might be as regarded England and Wales, they certainly could not be true of Scotland. He made inquiries, however, and found, to use his own words, that he, like many others, had been living in a fool's paradise, and that matters were not one whit better in Scotland than they were in England. And let it be remembered that the returns of the Registrar-General take no account of the vast array of non-fatal cases with their train of suffering and often of permanent ill-health, or of the many fatal cases that, for various reasons, have been attributed on the death certificate to some other than the true cause; but even taking the certified cases only, we have to face the unpleasant fact that the lives of more than 2,000 women are annually lost in England and Wales from a cause which is almost, if not entirely, preventable, and that puerperal fever continues to prevail, as though Pasteur and Lister had never lived. And if, notwithstanding all this, any one be inclined to urge that at least tragedies such as those which Holmes recorded are things of the past, let him refer to the second of Dr. Williams's Milroy Lectures for 1904, or to Mr. Foulerton's paper in the *Practitioner* for March, 1905, and he will be undeceived.

Some of these tragedies occur, no doubt, in the practice of ignorant and untrained midwives. Dr. Williams shows this to be strikingly the case in Glamorganshire, where most of the midwifery is in the hands of these women; but, as I showed in the presidential address already alluded to, we cannot, to use Dr. Milne Murray's striking figure, make the midwife the only scapegoat and send her into the wilderness bearing the whole burden. There is, I am afraid, no doubt that much of the general mortality from puerperal fever

* Writing in April of the present year (1905) Dr. Boxall, a well-known medical statistician, states that the death-rate from puerperal septic diseases has, if anything, shown a tendency to increase in each division of the kingdom (England, Scotland, and Ireland).

and even a certain proportion of the tragedies* occur in ordinary private practice.

After speaking of the lying-in institutions of this country, and of their condition being so vastly improved that their mortality from puerperal fever has "decreased almost to a vanishing point," Dr. Williams goes on to say that, such being the case, we are "forced to the conclusion that this undiminished mortality takes place outside these institutions—that is, in general practice—and that the conditions under which women are confined outside" have, for some reason or other, "not shared to an equal degree the improved methods and care adopted" within their walls.

This is not the occasion to discuss at any length the causes to which the persistence of puerperal fever in private is to be attributed. I have said my own say on the subject elsewhere, and will content myself to-day with calling attention to the extremely suggestive words uttered by Dr. Milne Murray in his Presidential address to the Obstetrical Society of Edinburgh in 1900. "Why," he asks, "are the results of private practice becoming worse and worse in spite of all that has been done for our science and art during the closing century? . . . I feel sure," he says, "that an explanation of much of the increase of maternal mortality from 1847 onwards will be found in, first, *the misuse of anæsthesia*, and second, *in the ridiculous parody which, in many practitioners' hands, stands for the use of antiseptics* . . . Before the days of anæsthesia interference was limited, and obstetric operations were at a minimum, because interference of all kinds increased the conscious suffering of the patient. . . . When anæsthesia became possible and interference

* In an introductory address, delivered at St. Thomas's Hospital in 1888, I stated that I knew of a country town not very far from London, where, within the preceding twelve months, puerperal fever had singled out the patients of one of the local practitioners exactly as it did in the instances I had been narrating. For six months every patient that he attended (with the exception of two in whom labour was over before his arrival, and whom, therefore, it was unnecessary for him to touch) had died of puerperal fever (*Puerperal Fever a Preventable Disease*, London, 1888, p. 32). The circumstances were at that time of too recent occurrence to permit of my giving the details, but I know of no reason why they should any longer be withheld. The facts, then are as follows: The wife of a labourer in a Sussex village was delivered with instruments some time in 1886 and died of blood-poisoning. Shortly afterwards a young woman, the wife of a master miller was attended in her second confinement by the same doctor, and died in ten days. In December, 1887, another labourer's wife in the village was confined, and died of blood-poisoning. The same doctor had attended her. On January 21st, 1888, at 9-30 a.m. he delivered a coachman's wife. Instruments were used, and the assistant was summoned to render additional help. The patient died of blood-poisoning at 9 p.m. on January. 29th. The nurse who had been in attendance along with the doctor on all these cases was now warned to attend no more cases for six weeks. The doctor continued his practice as usual, and so far as is known, adopted no means to rid himself of infection. On or about February 9th he attended a young farmer's wife, aged 23, the daughter of a clergyman, in her third confinement: she died of blood-poisoning on the tenth day. People now began to be afraid of employing the doctor, but, in the month of May he was called to attend a labourer's wife of middle age, who had last been confined nine years previously. Some difficulty occurred, and after endeavouring for some time, and with the help of two assistants, to deliver with the forceps he decided that the child's life must be sacrificed. Whilst the best means of procedure was being discussed a living child was born spontaneously. The mother, however, died of blood-poisoning at the end of ten days. These last four were all the cases attended by the doctor in question between December, 1887, and June, 1888, with the exception of two, in each of which he arrived too late, the nurse doing all that was necessary. One of these cases occurred in December, 1887, the other at the end of January 1888. Both of them escaped—a circumstance which was quoted on the doctor's behalf as a proof that he could not have been the source of infection in the fatal cases. Ultimately, however, he was prevailed upon to go away for a time, and shortly afterwards he left the neighbourhood. During the period covered by the above series of cases no case is known to have occurred in the practice of any other doctor (or of any midwife) in the district.

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became more frequent because it involved no additional suffering, operations were undertaken when really unnecessary, on the demand of the patient or for the convenience of the practitioner. And so complications arose and the dangers of labour increased. . . . Then came the antiseptic era. Here now was the panacea for all the dangers of childbed. All that was necessary was to dip the instruments for a few minutes in a carbolic lotion, and the hands in one of half the strength for half the time, and all danger was at an end. . . . Normal labour," he continues, "is a natural process which is best left to itself, and the less the patient is disturbed with the paraphernalia of obstetrics, before or after, the better. . . . Until men realise this and recognise the fact that the simplest obstetric operation demands not one whit less of care as to antiseptic precautions than is required of one before opening the abdomen, we shall get no further forward. When the practical obstetrician realises his responsibility, and that no small share of this terrible maternal mortality of a certainty lies at his door, he has made the first step towards true progress. When he realises that labour is a natural process which in the great majority of cases it is criminal to disturb; when he realises that every interference increases the inherent danger a hundredfold; and when under this consciousness he brings with him to the lying-in room all that is possible of those principles of antiseptic surgery which have been at the bottom of the triumphs of modern gynæcology, we shall not have long to wait for the lightening of the dark cloud which hangs over us now."

And with these weighty words of my deceased friend I must conclude. I trust that, the mortality from puerperal fever in Great Britain being what it is, you will not think it has been either ill-timed or out of place to recall that noteworthy chapter in the history of the subject of which it has to-day been my great privilege to speak. No sermon can be considered complete without at least a few words of application, and so I feel sure I shall be forgiven for attempting to apply the lesson of Holmes's essay to the circumstances of to-day, and to show that there is needed now, as there was needed then, a strong voice to rouse us from our lethargy, and to plead with desperate earnestness for the lives that are still being quite unnecessarily sacrificed.

"If," says Dr. James Jamieson, "anything in the field of practical medicine can be taken as proved, it is that puerperal fever is a preventable disease; and the means of prevention at our disposal are both more reliable and more easily accessible than in the case of

almost any other of the infectious diseases."* If that be true, and I for one firmly believe it is, surely we ought not to be satisfied until puerperal fever has been banished from amongst us.

APPENDIX.

Conclusions, including rules for preventing the spread of puerperal fever by infection, published at the end of Holmes's Essay (1843):—

1. A physician holding himself in readiness to attend cases of midwifery should never take any active part in the *post-mortem* examination of cases of puerperal fever.
2. If a physician is present at such autopsies, he should use thorough ablution, change every article of dress, and allow twenty-four hours or more to elapse before attending to any case of midwifery. It may be well to extend the same caution to cases of simple peritonitis.
3. Similar precautions should be taken after the autopsy or surgical treatment of cases of erysipelas, if the physician is obliged to unite such offices with his obstetrical duties, which is in the highest degree inexpedient.
4. On the occurrence of a single case of puerperal fever in his practice, the physician is bound to consider the next female he attends in labour, unless some weeks at least have elapsed, as in danger of being infected by him, and it is his duty to take every precaution to diminish her risk of disease and death.
5. If within a short period two cases of puerperal fever happen close to each other, in the practice of the same physician, the disease not existing or prevailing in the neighbourhood, he would do wisely to relinquish his obstetrical practice for at least one month, and endeavour to free himself by every available means from any noxious influence he may carry about with him.
6. The recurrence of three or more closely connected cases, in the practice of one individual, no others existing in the neighbourhood, and no other sufficient cause being alleged for the coincidence, is *prima facie* evidence that he is the vehicle of contagion.
7. It is the duty of the physician to take every precaution that the disease shall not be introduced by nurses or other assistants, by making proper enquiries concerning them and giving timely warning of every suspected source of danger.
8. Whatever indulgence may have been granted to those who have heretofore been the ignorant cause of so much misery, the time has come when the existence of a *private pestilence* in the sphere of a single physician should be looked upon not as a misfortune but as a crime, and in the knowledge of such occurrences the duties of the practitioner to his profession should give way to his paramount duties to society.

* Childbirth Mortality in the Australian Colonies, *Australian Medical Journal*, October 15th, 1887.

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NOTE.—This paper was followed by a discussion in which Simpson, Peddie, and others took part, bringing forward further evidence. See *Monthly Journ. Med. Sci.*, Edinburgh, June and July, 1851; also *Amer. Journ. Med. Sci.*, Oct., 1851.

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Dexter. *See* Hall.

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Although Dr. Cullingworth's article has already appeared in another place, the Editor asked the author's permission to reproduce it here, both for the intense importance of the subject, and in order to allow subscribers to the "Journal" to possess it in a form convenient for future reference.

Pudendal Myxoma.*

By G. F. Barbour Simpson, M.D., F.R.C.P.E., F.R.C.S.E., *Senior Assistant to the Professor of Midwifery, University of Edinburgh; Gynecological Tutor to the University Class of Clinical Medicine, Royal Infirmary, Edinburgh, etc.*

The vulva may at any time become the seat of some form of neoplasm, and while we not infrequently meet with such varieties as the caruncle at the orifice of the urethra, various forms of specific swellings (warty or gummatous in nature), lipomata, fibromata and carcinomata, it is rare to meet with a tumour in this situation of a purely myxomatous type. I desire, therefore, to place on record the following case.

The patient, a Jewess, age 16, came to see me in May, complaining of a growth in the privates accompanied by bleeding. Shortly before leaving school at the age of 13, she noticed a small swelling about the size of a plum at the vulva. At first sessile, it gave rise to inconvenience but no pain was experienced. The swelling gradually increased in size and with this increase in size began to hang down and become pediculated. A year ago the patient began to experience difficulty in locomotion owing to the growth coming in contact with the thighs. Latterly, as the surface of the tumour showed signs of excoriation, bleeding set in, not much at a time, and usually at intervals of a week. Of late, owing to the hæmorrhage becoming more frequent and the general health beginning to be undermined, she was glad to seek medical aid. There is nothing of importance to note in the previous health, family or social conditions of the patient. (Figs. 1 and 2.)

Menstruation set in at 12½ years, regular in type and habit. On examination of the external pudenda a tumour 4 inches in length and greatest breadth 4 inches was observed growing by a stalk 1½ inches broad from the right labium majus; it was uniform above and lobulated below where it was also excoriated and slightly fungating. Marked pulsation was felt in the stalk of the tumour.

Treatment. The pedicle was grasped near its base by means of clamp forceps and divided. The larger vessels were ligatured with catgut and the clamp forceps gradually slackened until all bleeding points were secured. The raw surfaces were then united by means of a continuous catgut suture and a dressing of gauze applied externally. The catheter was passed for the first forty-eight hours after the

* Read before the Edinburgh Obstetrical Society. November, 1905.

operation to prevent any contamination of the wound, and the patient was able to go about perfectly well three weeks later. I saw her again yesterday and on examination found the scar perfectly healthy.

On macroscopic examination the whole of the external surface of the tumour is seen to be covered with skin epithelium and has a distinctly lobulated appearance. The masses vary in size from about 2 c.m. in diameter to small papillary outgrowths of about $^1/_{10}$ c.m. (Fig. 2.)

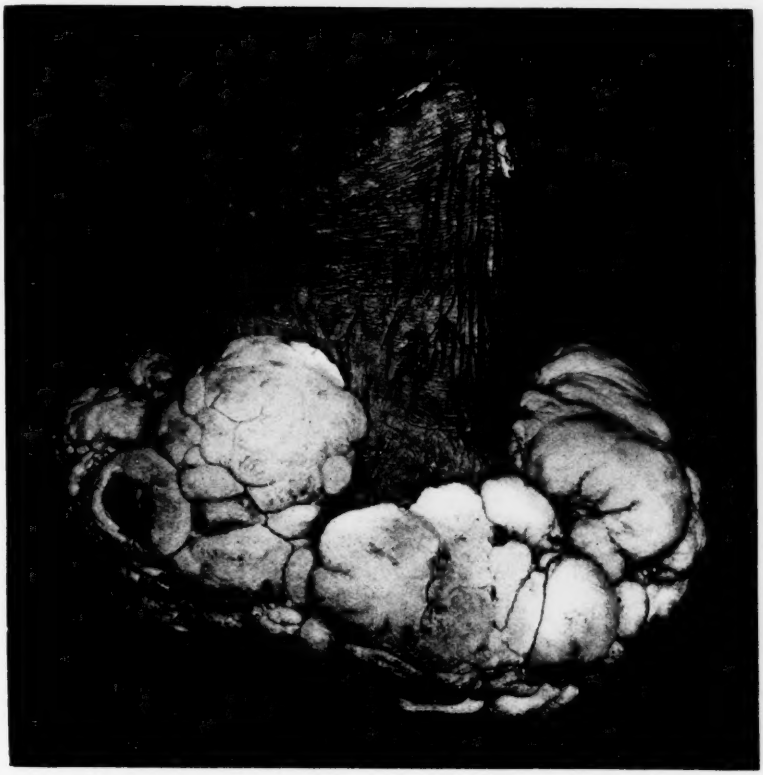
The epithelium over the larger masses appears to be stretched, and through it one can see the swollen papillæ which have the appearance of sago grains. The growth is firm and elastic to the touch, as if filled with firm gelatinous material, at other parts the surface shows typical papillary outgrowths with thickened epidermis. Section through the growth shows the tumour to be mostly composed of a clear gelatinous material, through which is seen running strands of well-formed fibrous tissue cutting up the jelly-like substance into various sized masses. This gelatinous material is more abundant at the centre of the growth, but is also seen in parts to have spread right out to the epithelium, distending that part and giving rise to the raised lobulated appearance of the surface. At some parts the gelatinous material contains blood vessels, at others, it appears to be quite devoid of vessels.

Histologically, this gelatinous substance is seen to be myxomatous tissue, showing typical branching cells with their delicate processes ramifying amongst the mucoid substance, and in parts young embryonic blood vessels are evident. Section through one of the larger masses at the surface of the growth shows many of the papillæ greatly distended and deformed from marked changes in the stroma, which is undergoing both œdematous infiltration and myxomatous degeneration, causing stretching and atrophy of the surface epithelium. At various points the subcutaneous tissue shows fibrous overgrowth with portions here and there of commencing myxomatous softening. Many of the arteries show thickening of their walls and the veins are prominent and distended with blood. The lymphatics are numerous, the larger lymph channels containing granular material. Many mononucleated cells are seen lying throughout the subcutaneous tissue, but more especially in the perivascular lymphatics.

Transverse section through the stalk shows it to be composed of the normal elements and arrangement of the hypertrophied skin. In the centre of the stalk and running through two-thirds of its length is seen a mass of myxomatous tissue, sharply defined from the tissues in which it lies and from which at one point in particular

PLATE I.

PLATE II.



blood vessels are to be seen passing from it into the myxomatous mass, and giving rise to new blood-vessels forming vascularised buds—apparently organisation of the foreign material.

Virchow distinguished different subdivisions of myxoma, viz., *myxoma gelatinosum* which is largely supplied with homogeneous intercellular substance; *myxoma fibrosum* with a great development of fibrillar intercellular substance; *myxoma medullare*, distinguished by the increase of its cellular elements and forming the transition to the corresponding cellular form of tumour, the myxo-sarcoma; *myxoma teleangiectaticum* furnished with numerous capillaries; *myxoma cavernosum* containing many cavernous blood vessels. Of these subdivisions, the foregoing case would come to be included under the myxoma gelatinosum.

While myxoma is not infrequently met with in the mamma and in other glands such as the salivary glands and the testicle, it is extremely rare to meet with this disease in association with the external organs of generation in the female. Aschenborn[1] records a case of a patient age 35 who was the subject of a tumour growing from the labium majus for four years, which on removal by extirpation was examined microscopically and found to be a myxoma. Atthill[2] also describes an interesting case of a 3-para, with enlargement of the left labium. At the time of observation the woman was pregnant, the tumour being the size of a walnut. After birth of the child, the swelling of the labium disappeared. This was, however, but for a time, for the labium soon began to increase in size till at the end of nine months it had become so large as to cause great inconvenience. The growth was enucleated and found microscopically to belong to the class of myxomatous tumours. Suchard[3] relates a case of a patient, age 35, who had a swelling of the labium majus of two years duration, unaccompanied by pain. At each menstrual epoch there was congestion and increase in size of the tumour, which became pediculated and reached the size of a fist, and was removed. Macroscopically it was observed to consist of soft gelatinous tissue, and microscopically presented the appearances of a pure myxoma.

It would thus appear that such tumours may develop early in life or not until a later period, that they seldom give rise to pain, but tend to increase steadily in size, and that operative interference is indicated in all cases as giving most satisfactory results.

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On the Use of Gloves in Abdominal Operations.*

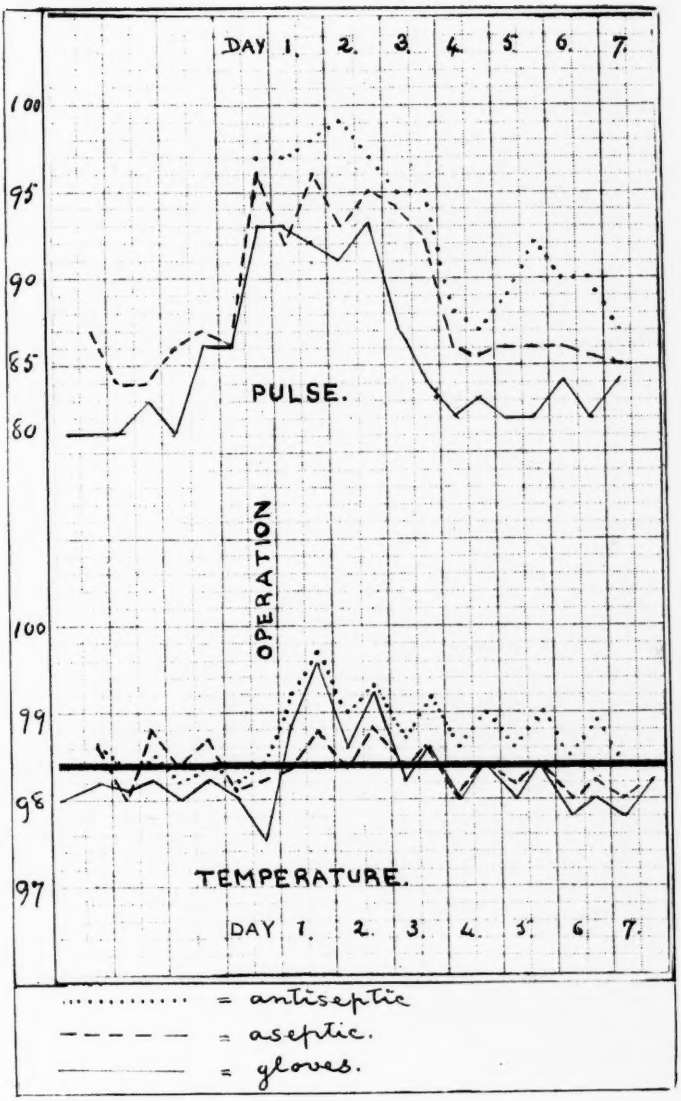
By T. B. Grimsdale, B.A., M.D.,
Hon. Gynæcologist, Liverpool Royal Infirmary; Lecturer on Gynæcology, University of Liverpool.

The results of operations have become so good during the last ten years or so, owing to the improvements in operative technique, that mortality has almost disappeared, and very few patients indeed die even from the most formidable surgical operations; so that the old test of success, the mortality test, is no longer of any value as a delicate test of further improvements in operative methods. The test which is of value, is one which brings out and demonstrates the ease with which a patient recovers after operation. Such a test as this I wish to lay before you to-night so that you may judge whether it is advisable to use gloves during operations or not.

For some years many surgeons have performed all their operations in rubber gloves, whilst others have not considered it at all necessary to do so. Some have laid great stress on the disadvantages of gloves and their inconvenience, and call attention to the impairment of the sense of touch. Others look on gloves as a useful protection for themselves, and use them only in dirty cases. I myself can see no disadvantage in the use of gloves. If the sense of touch is impaired it only means that we must work more by sight. This seeming disadvantage only emphasises the surgical axiom, "See what you are doing." With a very little practice gloves will be found to be no detriment to the operation. The difference of opinion on this question has only been tested, so far as I know, by a statement of general impressions which are of no real value in judging of a method.

Some surgeons may, and do, get very good results without using gloves, better indeed than others who do use gloves, but I think it is certain that these very good results would be improved if these surgeons consistently performed all their operations in gloves. The only fair way in which a comparison can be made on this point is to compare the results of the work of the same man. In order to give you this test and offer you some definite facts and figures, I have made some composite charts of temperatures and pulses recorded during the recovery of patients after abdominal operations, so that you may make a comparison between the different methods used by myself in 120 abdominal operations, divided into three series of

* Read at a meeting of the North of England Obstetrical and Gynæcological Society, November, 1905.


DAY 1. 2. 3. 4. 5. 6. 7.
100
95
90
85
80
PULSE.
OPERATION
100
99
98
97
TEMPERATURE.
DAY 1. 2. 3. 4. 5. 6. 7.
..... = antiseptic
– – – – = aseptic.
——— = gloves.

40 cases in each series.* The three series of cases show the results of the antiseptic, the aseptic and the glove methods.

The operations were all performed by me at the Liverpool Royal Infirmary, and each series was a consecutive series of 40 abdominal operations. These operations were performed for the ordinary run of gynæcological diseases, so that the comparison is as fair as it is possible to make it.

I need not describe in detail the exact methods I adopted in operating in the three series of cases, further than to say that in the antiseptic series antiseptics were used during the operation; the instruments were placed in antiseptic solutions, and antiseptic dressings were used. In the aseptic series no antiseptics were used at the operation. The glove series was exactly like the aseptic series plus the use of gloves.

There is one point I must draw special attention to, and that is that, in the aseptic series, the greatest care was taken in cleaning the hands the permanganate of potash and oxalic acid method being used most religiously, and on various occasions our hands were tested by three different competent bacteriologists, and they invariably declared our hands to be sterile after the most searching examination. Yet in spite of this, our results are improved by using gloves after the same rigorous cleansing of the hands. This rather supports the contention, which I have combatted, that it is impossible to thoroughly sterilise the hands.

The composite charts are made in the following way:—Two observations were taken each day in each case—one at 10 o'clock in the morning, and the other at 6 o'clock in the evening. In the composite charts I show you each reading is the mean of 40 observations. For instance, in the composite pulse chart (Fig. I.), in the day space marked operation the first black dot on the dotted line records 97; that means 97 beats per minute at 6 o'clock on the evening of the day of operation is the average pulse-rate of 40 patients who were operated on by the antiseptic method. Of course this record applies to work done many years ago. The broken line represents the average pulse-rate of 40 patients operated on by a strict aseptic method. The unbroken line represents the average pulse-rate of 40 patients operated on by exactly the same method plus the use of gloves. I think you will see at a glance that the broken line is better than the dotted line, and that the unbroken line is all through the best of all as far as the pulse is concerned.

* In the last series one case is left out. Ruptured uterus: died half an hour after operation.

The temperature charts are much the same for each series. The pulse is evidently a more delicate test than the temperature of the ease of the recovery; with this all will agree. You will notice that on the third day after operation in the glove series (the unbroken line) the pulse-rate has practically returned to normal, whereas the other two series require another day. There is the gain of a day when gloves are used. It is interesting and gratifying, in making these comparisons, to find that there has been a steady improvement in the convalescence of the patients after operation with each change in the method of operating.

As far as I myself are concerned, I am perfectly convinced that by the use of gloves the operative results are very greatly improved, and I hope that by showing you these figures I may likewise prove, to some who do not consider it necessary to use gloves, that their results will be improved if they do use them. It is easy to say, as has often been said to me, that "results are quite good enough without bothering with gloves." There is no doubt in my mind that results will be better with gloves, and it is a pity to be content with anything short of the best you can attain.

To go back to the pulse charts. If you follow the dotted line (the antiseptic series) on the pulse charts I think you will say the results are good; the highest average pulse during the convalescence is only 100, but the other two lines are better. I might say that the number 40 for each series was taken quite arbitrarily. I might have taken 10, 20 or 100 in each series, but the labour entailed in making these average composite charts is considerable. I might remind you that each point on these charts means 40 observations, 40 additions, and a division by 40, and I think that a series of 40 cases is a fair number and sufficient to exclude grave errors.

A Case of Eclampsia with Two Special Details of Treatment.*

By C. Nepean Longridge, M.D. (Vict.), F.R.C.S. (Eng.), M.R.C.P. (Lond.), *late Resident Medical Officer at Queen Charlotte's Hospital.*

A fair woman, aged 25, and somewhat nervous about herself, was seen in the O.P. Department of Queen Charlotte's Hospital in the fourth month of her second pregnancy. She had had eclampsia during her first labour, the infant being still born. The urine was free from albumen, there was no œdema, and no symptom of the toxæmia of pregnancy. Vomiting had been slight during the second month. The blood pressure in the right brachial artery taken by the Riva Rocci apparatus was 140 mm. of Hg. She was seen at intervals during the pregnancy, and the urine on all occasions was found to be free from albumen and the general condition good. For the last month she was sent to a home where she could be under supervision. On August 31 she complained of headache, slight vomiting and diarrhœa (15 actions). At 10-30 p.m. she was brought to the hospital; immediately on admission she had a fit and became unconscious. Two fits followed in rapid succession, the duration of each being about three minutes. The size of the uterus corresponded to that of a full-time pregnancy, the fœtal heart was heard and the child was lying in the L.O.A. position with the head well engaged. The cervix was short and admitted two fingers, the membranes being unruptured. The blood pressure was 160 mm. of Hg., the urine became nearly solid on boiling; there was some œdema of the feet and legs.

Five grains of calomel and $\frac{1}{4}$ grain of morphia were given. The calomel was vomited at once, and replaced in about half an hour by an ounce of oil. She had four more fits between one and three o'clock, when another $\frac{1}{4}$ grain of morphia was given. There was marked cyanosis during the fits, and deep unconsciousness between them. At 6-30 a.m. the os was found to be fully dilated, the membranes were therefore ruptured, forceps applied, and the child delivered. No attempt was made to check bleeding, which amounted to 12 ounces. She had a fit during the birth of the placenta. From 7—10-30 a.m. she had six fits. A third injection of morphia was

* Read at the Obstetrical Society of London, November, 1905.

given at 9-30. From 10-30 to 11-30 four fits occurred of greater severity and duration than any which had hitherto taken place. As the patient's condition appeared to be getting rapidly worse, the right median basilic vein was opened and about 10 ounces of blood removed and 32 ounces of saline run into the vein. The stomach was washed out with warm water, half a pint of saline, 5 ounces of milk, and an ounce of oil being left in. In the next two hours three fits occurred. The bowels being not yet open, an enema containing an ounce of magnesium sulphate and half an ounce of glycerine was given, together with 2 m. of croton oil by the mouth; there were two fits in the next hour. She was then given occasional whiffs of chloroform, being kept warmly wrapped up with hot bottles and blankets. At 3 o'clock profuse perspiration began and continued, and some water was passed. Two more slight fits were noticed about two hours later. At 6-30 as the pulse was very rapid and feeble, a fourth injection of morphia was given, and two pints of saline with an ounce of brandy by the bowel. The patient soon improved and became less unconscious, and was able to swallow half a pint of albumen water. At 10-40 p.m. the bowels acted for the first time to the accompaniment of a slight fit, this being the 25th and the last. During the day the œdema increased rapidly, the hands and face being particularly affected. The pupils remained firmly contracted, except during the fits, when they dilated. During the night she was restless, took plenty of sweetened milk and barley water, and passed urine. Ten watery foul-smelling motions were passed. The temperature which, at the cessation of the fits, was 105°, fell in the morning. There were no pulmonary complications. The patient remained unconscious from 10-30 p.m. on Aug. 31st until about 6 a.m. on Sept. 3rd. She suffered from a marked loss of memory, which was not completely restored six weeks after her illness. On Sept. 4th she developed jaundice which lasted until Sept. 9th; there were no hæmorrhages. At the same time she had some labial herpes and itching of the head. On Sept. 4th the œdema had practically disappeared and the blood pressure was reduced to 120 mm. Hg. The urine contained about $\frac{1}{3}$ albumen and a large quantity of casts. The albumen had disappeared by the sixth day. Subsequent progress was uneventful and the infant did well.

The day after delivery the alkalinity of the blood was examined by Wright's[1] method and found to be equal to $\frac{N}{75}H_2SO_4$ the alkalinity of healthy blood being $\frac{N}{35}$ H_2SO_4. A mixture containing magnesium sulphate one ounce, citrate of soda half a drachm, and liquid extract of

cascara half a drachm, was given every three hours. A pint of saline containing an ounce of lactose was injected into the rectum every four hours. Citrate of soda was given to increase the alkalinity of the blood. In two days the alkalinity was brought up to $\frac{N}{30}$ H_2SO_4. The first estimation of the alkalinity was only made twelve hours after the fits had ceased, and a considerable elimination of toxin had taken place. In another mild case of eclampsia where the patient had four fits the alkalinity was found to be $\frac{N}{80}$ H_2SO_4, while the fits were occurring. A third case gave an alkalinity of $\frac{N}{60}$. This detail may be an important one as showing that the intoxication, whatever it is, is probably an acid one, and as an indication for the exhibition of alkalies. A citrate was given as it is absorbed readily into the blood as a carbonate, and so increases the alkalinity. Jardine,[2] a strong advocate for transfusion in these cases, has recommended the addition of one drachm of sodium acetate to the pint of normal saline for transfusion, with a view to securing diuresis, and not with the intention of increasing the alkalinity of the blood, but it is possible that his excellent results are partly due to the increased alkalinity induced by his method.

A second detail of treatment rests on a highly theoretical basis. The patient was given considerable quantities of sugar by the mouth and rectum. None appeared in the urine. The patient became markedly jaundiced, showing that the liver had gone through a period of stress, and one imagines that the bile canaliculi became blocked with the débris of the storm, in much the same way that the tubules of the kidney became choked with casts. The work of Schiff, Bouchard and Roger[3] has proved that the liver possesses an antitoxic function. It seems that this antitoxic function can only be exercised in the presence of glycogen, and further that the power of the liver to decrease the toxicity of poisons depends directly on the amount of glycogen contained in its cells. It was thought in this case that in consequence of the high temperature, and the severe muscular exertion which the patient had undergone, the glycogenic content of the liver was practically exhausted, and sugar was given with a view of increasing it, and so increasing the antitoxic power of the liver. Three years ago the author advocated the use of glucose in feeding typhoid patients on these grounds,[4] and his small experience as a House Physician led him to regard the results with favour.

It is probable that transfusion saved the life of this patient, and the case illustrates the arguments urged by Jardine in favour of this

line of treatment. Of the twenty-five fits, seven occurred before, one during, and seventeen after, delivery. Ten of these seventeen occurred in five hours before transfusion and were increasing in severity. Seven occurred in ten and a half hours after transfusion, the last three being slight in character. It is of course impossible to say how far the two details of the after treatment which have been described contributed to the rapid recovery of the patient, but they appear to rest upon a thoroughly scientific basis.

The author is indebted to Dr. Griffith for permission to publish this case.

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Three Cases of Injury of the Parturient Canal.

By W. E. Fothergill, M.A., B.Sc., M.D., Manchester.

The following simple records of injuries of the parturient canal may be of interest to those who are collecting the statistics of ruptures and lacerations of the uterus and vagina. They illustrate the facts that under certain circumstances injuries occur in cases in which there is no serious cause of dystocia, and that lacerations may be produced at the fifth or seventh month of pregnancy as easily as at term.

Case 1. Mrs. X.Y., aged 42, had had six children previously. She suffered from melancholia during her seventh pregnancy, but progressed fairly well under treatment, and labour at term began on September 24th. I am informed that when she was examined at 3 p.m. the head was fairly low down, and the rectum was loaded with fæces. After an enema several fæcal masses were passed. Labour pains continued without much progress until 10-30 p.m. Under chloroform the position was found to be left occipito posterior. The head was rotated and easily delivered with forceps, but the delivery of the shoulders was only accomplished with considerable difficulty. The child was a large one, and the head was unusually well ossified. The placenta was retained and on introducing the hand to remove it a tear in the posterior vaginal fornix was discovered, through which several loops of bowel protruded. These were returned into the abdominal cavity, and, the secundines having been removed, the vagina was packed with iodoform gauze.

I saw the case in consultation at 5 p.m. on the following day, September 25th. The patient was then rational and cheerful, and complained of no pain. She had a slightly subnormal temperature, and the pulse was small and running at the rate of about 150 per minute. The tongue was clean and moist. The abdomen was greatly distended, and was universally tympanitic; there was no liver dulness nor was there any uterine dulness. On removing the packing, the vagina was found to be so enormous that it was almost impossible to get a view of the tear. Inserting the hand into the vagina, the posterior portion of the cervix was felt to be contracted upon the posterior uterine wall as a firm irregular mass, below this the hand passed through a transverse tear extending across the vaginal vault straight into the rectum. This was distended by solid fæcal masses. The hand, in the peritoneal cavity, was passed up over the sigmoid flexure, which, with the whole of the descending colon, was filled with hard fæces forming a mass suggesting in length and thickness

the arm of a big man. There was some blood in the peritoneal cavity, which was washed out with a stream of normal saline solution.

The patient's condition of collapse was such that any major operation appeared certain to prove immediately fatal. There was, however, a bare possibility that if free drainage were maintained recovery might follow. The vagina was therefore packed lightly with iodoform gauze, and it was arranged that treatment should be directed towards mechanically emptying the colon, should the patient's condition improve sufficiently to admit of the necessary lavage and manipulation. I did not see the case again, but I am informed that the next day the distension was if anything greater. Aperients and enematæ were given without effect. Vomiting was continuous, and was not stercoraceous but bilious. On September 28th she took iced champagne and small quantities of milk. Vomiting continued, but the bowels began to act. Strychnine was administered in small doses. On the 30th rectal feeding was tried. The patient's aspect was pinched, her pulse was small and uncountable, the temperature was 100°. There were signs of congestion at the bases of both lungs. On October 1st she was able to take milk and was conscious and rational, but she collapsed suddenly at 9-30 p.m., and died at 10-45 p.m.

The determining cause of the laceration in this case was certainly the presence of large fæcal accumulations in the rectum and colon. The enema given during labour brought away a copious motion, but a vast mass was left behind and formed an obstruction to delivery. The tear would probably have occurred ultimately apart from the delivery by forceps. For there must have been a predisposing cause, namely unusual friability of the tissues. The fact that the patient was suffering from melancholia is of interest, as it indicates a condition of toxæmia which might well have some causal relationship with the fragile state of the parturient canal, and which certainly explains the neglected state of the patient's rectum and colon.

Case II. Mrs. A.B., aged 37, has been pregnant sixteen times, and has eleven living children, of whom the youngest is two years old. She has had one still-born child and four miscarriages, one of which occurred at the third month since the birth of the last child. She last menstruated at the end of April, 1905. At the end of July, some bleeding occured, and hæmorrhage has continued with intermissions ever since. Since September 16th she has been confined to bed, and has complained of "shivering and burning." On September 22nd a fœtus was expelled, which was said to correspond in development to the duration of pregnancy, namely almost nineteen

weeks. This occurred at 3 p.m. As the secundines did not come away a doctor was sent for and arrived at 8 o'clock p.m. The patient, who was an alcoholic, was very restive, and the doctor informs me that after experiencing very great difficulty in making an examination, he anæsthetised the patient with some trouble. He then succeeded in passing two fingers into the uterus, and after prolonged bimanual manipulation separated the adherent placenta and removed it together with the membranes. He remembered noticing an abnormal posterior fornix, but did not examine it, being intent on controlling the patient and emptying the uterus. As the pulse and temperature continued unsatisfactory he sent the patient to Monsall Hospital.

Soon after her arrival Dr. Gordon removed several decomposing blood-clots from the uterus and vagina under anæsthesia. A rent in the vagina wall was then found, crossing the posterior fornix and extending forward across the left lateral fornix. The tear led into a cavity filled with decomposing blood-clot. This was removed so as to permit of cleansing of the cavity, which was then packed with iodoform gauze. I saw the case with Dr. Gordon the same evening, September 30th. We found the temperature high and the pulse rapid, the patient being in a state of intense rigor at the time. When this had passed off we made a further examination in order to form an opinion as to the need for further interference.

We found the abdominal wall soft and lax. There was no tenderness on deep palpation, involution appeared to have progressed normally. The cervix was large and soft, and easily admitted a finger. On exposing it and pulling it down with a volsella, the cervical canal was seen to be intact. There was a grooved wound an inch long and $\frac{1}{8}$ inch deep, crossing the vaginal surface of the anterior lip. The vagina was cavernous in size, and it was only on pushing out of the way its lax posterior wall that the posterior lip of the cervix was seen to be separated from the vaginal wall by a tear crossing from the right fornix round the back of the cervix, and extending well forward into the left fornix. The finger could be passed for its whole length into the cavity above the tear. The peritoneum appeared to be stripped off the lower two inches of the back of the uterus, and the left broad ligament was split extensively. No opening into the peritoneal cavity could be felt. The cavity was lightly packed with iodoform gauze, some of which was also passed into the cervix.

It was thought that if the peritoneal cavity had been opened it must now be shut off by adhesions, and it was considered that as all

was now clear, and drainage was free, there was no indication for further interference. The patient made a good recovery.

Case III. The notes of this case were sent to me by a friend as an illustration of the accidents which are possible in connection with the use of Champetier de Ribes' bag. To show that he was experienced in this method of dilatation I may mention that he had previously used de Ribes' bag seven times. In five cases the object was the induction of premature labour for contracted pelvis in women who had previously been delivered by craniotomy. In the other two cases the indications for dilatation were eclampsia and placenta prævia respectively. In these seven cases the use of the bag was quite satisfactory, and was not followed by any septic trouble.

In my friends' eighth case, the patient was a woman who had had five children. After the fifth month of her sixth pregnancy she had a constant sanious discharge for two months. When she was first seen at the end of the seventh month she had had a rigor, the fœtus was found to be dead, the membranes had ruptured, and all the liquor amnii had escaped. Her temperature was 101·2°, and there were no pains. It was therefore decided to induce labour at once. Using strict antiseptic precautions, my friend dilated the cervix with Hegar's dilators, until a Champetier de Ribes' bag could be introduced. This was left *in situ* for eight hours. As labour pains did not come on gentle traction on the stalk of the bag was begun at the end of that time and was continued until dilatation was complete, when the bag escaped into the vagina. The fœtus, which was "absolutely rotten" was delivered piecemeal, and the uterus was emptied and washed out.

On the following morning the temperature was 103°, and my friend, thinking that the uterus might still contain some placental debris, gently introduced a blunt curette and pulled down some tissue which he at once recognised as maternal omentum.

Being afraid to return this omentum from the foul uterus into the peritoneal cavity, he opened the abdomen and ligatured the portion of omentum above the point where it entered the uterus. After cutting through the omentum between the ligature and the uterus, he found that the uterine rupture was transverse in direction, that it was just above the cervix in the lower uterine segment, and that it measured about 1½ inches in length. He pushed the fragment of omentum through into the vagina, drew together the ends of the rupture, and passed a gauze drain through the centre of the tear into the vagina. The abdominal incision was then closed. An abscess subsequently formed in the peritoneal cavity, but the patient's

friends would not allow any further operation, and the abscess discharged both through the abdominal wound and through the uterine rupture. This delayed recovery somewhat, but the last time my friend saw his patient she was actively engaged at the family wash-tub. He thinks the rupture may have occurred when he was putting the bag in, the uterus being pushed up from the cervix, or again when he pulled the bag out, the cervix then being pulled down from the uterus. At the seat of rupture the uterine wall was the thickness of a shilling. The injury might doubtless have occurred during the extraction of the fœtus or of the placenta, but was probably connected with the use of the bag. My friend concludes from this case that some uteri require very tender treatment, and he modestly adds that "it is better to be born lucky than skilful."

Cases ii. and iii. have certain common features. Thus in both there was prolonged hæmorrhage, followed by the premature termination of pregnancy at the end of the fifth and seventh months respectively. In each case the fœtus perished before delivery. It is probable that the bleeding in each case was rendered possible by the same defect in the uterine tissues which was the predisposing cause of the lacerations described. Microscopic examination of the uterine wall has revealed definite changes in some cases of rupture, though not in all. In some cases of accidental hæmorrhage a similar weakened condition has been described. In all probability a toxæmia of one kind or another is the underlying feature common to the majority of these cases. The tissues of the parturient canal may be thinned and stretched by multiple parturitions. In the second case mentioned, for instance, the patient had been pregnant sixteen times. The extreme degree of friability observed in these and similar cases must, however, be attributed to some cause quite apart from repeated parturition. When this diseased state of the tissues is encountered removal of the uterus is obviously preferable to repair of the rupture or laceration for cases in which abdominal section is required.

SECONDARY REPAIR OF COMPLETE PERINEAL LACERATION.

To the Editor of the "Journal of Obstetrics and Gynæcology of the British Empire."

SIR,—

In an article with the above heading in the current number of the JOURNAL (November, 1905), Dr. C. G. Child, jun., of New York, refers in complimentary terms to a paper of mine published in the July number. Dr. Child, however, has misinterpreted the appearances in drawings 3 and 4 accompanying my paper. He says that in each of them "both vaginal and rectal flaps are vertically incised for a considerable extent." This is not so. What Dr. Child has taken for incisions are the tears extending for some distance up the recto-vaginal septum, and therefore visible as a more or less linear slit in both rectum and vagina when the two layers of the septum are separated by the process of "splitting" or "dissecting up" the flap. *I make no incisions whatever in either the rectal portion of the divided flap or the vaginal portion.*

As this is the pivot on which Dr. Child's communication turns, I shall be glad if you will allow me to make this correction.

Yours sincerely,

C. J. CULLINGWORTH.

Manchester Square, London, W.
November 11th, 1905.

P.S.—In the drawings accompanying Dr. Child's paper the sphincter alone is torn, not the recto-vaginal septum.

REVIEW OF CURRENT LITERATURE.

OBSTETRICS.

A Case of Puerperal Osteo-malacia, Cæsarean Section, and Bilateral Oöphorectomy: Relapse.

ROSSIER (A.). *Annales de Gynécol. et d'Obstèt.*, August, 1905.

THE patient was a young married woman of 29, who had borne two children at term. Both these labours were normal. The history dated back about four years, when she entered the maternity at Bâle pregnant again at full term with all the signs and symptoms of osteo-malacia. The skeleton was markedly deformed, the diagonal conjugate was $3\frac{1}{4}$ inches, and the available obstetrical conjugate only $1\frac{3}{4}$ inches. The pelvis was of the crumpled up type, and the symphysis remarkably beaked. Pressure on the pelvic bones was attended with much pain. Cæsarean section was performed in the usual way, and a full-term child delivered alive. Both ovaries were removed, and the pedicles secured by silk. The uterus was closed with catgut; the peritoneum with a continuous gut suture, the muscles and fascia with interrupted gut and silk sutures alternately, and the skin with interrupted silk. The patient seemed to progress well, the skeletal pains diminished and her general health improved after the operation. She left the Hospital two months later.

Thirteen months subsequently the patient was re-admitted. Apparently the disease had not been arrested by the castration, nor had the periods ceased. Pains in the limbs, and increased difficulty in walking had been worse for six months before her re-admission. She now suffered very severely, and was quite unable to walk or even move. Measurements, both of the pelvis and skeleton showed that there was increasing deformity of the bones. The case seems one of some interest, though very disappointing to the operator, for even after what was thought to be complete removal of the ovaries relapse occurred, and the menstrual periods continued. The question now arose as to what was best to be done, and as the patient begged for some relief, a second laparotomy was performed (*i.e.*, 13 months after the Cæsarean section), and in the right broad ligament a small swelling was found, which on microscopic section revealed ovarian tissue. This was removed. The patient stood the operation well, and left the Hospital six weeks after. Later reports,

however, are disappointing, and the disease seems to be advancing, in fact Rossier looks upon the whole case as sporadic osteo-malacia of a very malignant type.

With regard to the first operation and removal of the ovaries, as far as could be determined at the time both ovaries were completely taken away. Microscopic examination of the removed ovaries showed markedly thickened vessels undergoing hyaline degeneration. In places the blood-vessels were very numerous and even hypertrophied. The connective tissue stroma of the ovary also showed hyaline degeneration. Next, taking into account the recurrence of the disease, and the failure of the first operation to suppress menstruation, Rossier takes it that either the disease was of a very malignant type, or ovarian tissue still existed at the time of the second operation. The rounded body found and removed at the second laparotomy was carefully microscoped and found to contain ovarian tissue—corpora albicantia, a small follicle cyst, but no ovules. Could this be part of the right ovary incompletely removed, or was it really a third or accessory ovary? The author inclines to the latter hypothesis.

L. Seitz has lately written on this very difficult question, and concludes that ovaries such as those described, are either supernumerary or accessory. *Supernumerary* ovaries exactly comparable to the two other ovaries are very rare, and only accompany doubling of the tubes, and, in fact, are a deformity *per excessum*.

Supernumerary ovaries, according to Seitz, may result (a) *during fœtal life* by budding out of the connective tissue and inclusion of ovarian substance. They are retro-peritoneal, if such separation occur early, intra-ligamentous, if that peritoneal fold is already formed, or lastly intra-peritoneal, either completely or partly separated from the normal ovary itself. (b) Accessory ovaries may arise in the *post-fœtal period* of life by pathological processes.

Finally it sometimes happens that accessory ovaries result from remains of primordial epithelium, either nests of ova, or primitive follicles, being included in the broad ligament itself

Cases of a somewhat similar nature where pregnancy has followed what was believed to be complete removal of both ovaries have been reported in England by Meredith and Doran.

With regard to the question of conservative Cæsarean section and oöphorectomy *versus* Porro's operation in cases of osteo-malacia, Rossier prefers the former, reserving removal of the uterus for cases definitely infected before admission or where the risk of infection has taken place.

C. Hubert Roberts.

Labour in a Case of Uterus Duplex Separatus and Vagina Septa.

POLLAK. *Archiv für Gynäkol.* Bd. lxxv., Ht. 2.

THE patient was a primipara aged 19 years. The presentation was a footling, and manual extraction was employed. The mother recovered, but the child was still-born, being asphyxiated by coils of cord around the neck. The vaginal partition was thick and fleshy. Measurements showed a wide pelvis. The inter-spinous diameter was 29 c.m., the intercristal 31·5 c.m., and the transverse at the outlet 11·5 c.m. The round ligament of the gravid left uterus ran vertically on the anterior surface of the uterus. To Pfannenstiel's twelve cases the author adds his own and eight others collected from the literature on the subject.

The author devotes a good deal of space to a discussion of the theories advanced to account for the non-union of Müller's ducts. The significance of the recto-vesical ligament, persistent allantois, and fœtal peritonitis are discussed. The modern view seems to be that not one but various causal factors come into play in the production of uterus septus and bipartitus. Paltauf speaks of dysplasia and aplasia of the uropoetic viscera. Tiersch has drawn attention to the posterior displacement of the lower parts of the Wolffian bodies. Pick's view as to the incorporation of aberrant cell-groups and the development of myomata in the septum of a uterus septus is well known. The author of the present article inclines to Pfannenstiel's view and accepts a primary "germ-anomaly" as the cause of the wide separation of the pelvis and its contained structures generally. Pfannenstiel drew attention to the wide pelvic measurements in these cases, and regards the width of the pelvis as accounting for the failure of union of Müller's ducts. Pollak found that in thirteen cases, five of them, *i.e.*, 38 per cent., were associated with abnormally wide pelves.

CUTHBERT LOCKYER.

The Internal Secretion of the Ovary and Placenta and their Influence on the Secretion of Milk.

HALBAN. *Archiv für Gynäkol.* Bd. lxxv., Ht. 2.

THE author states that the changes in the mamma incident to puberty are dependent upon material elaborated in the ovary. The menstrual changes in the breast are likewise caused by a chemical substance obtained from the ovary.

During pregnancy the ovaries are not the trophic centre of the internal genitalia and the breast, as at other times they appear to be. In pregnancy the trophic influence of the ovaries is insignificant.

The ovaries have no influence on the hypertrophy of mammary tissue, and on the milk secretion which occur during pregnancy. The breasts develop and secrete milk after complete castration performed in the gestation period.

The ovum is independent of the pregnancy-reaction (der Auslösung der Schwangerschaft's reaction).

The active pregnancy-substances are the products of the placenta, *i.e.*, of the trophoblast or chorionic epithelium.

The changes in the breasts accompanying gestation are the result of the effects produced by these substances derived from the trophoblast.

Both placental (trophoblastic) and ovarian substances have the common property of exciting hyperæmia and hæmorrhage. The action of the two substances is analogous, but the effect of that derived from the trophoblast is more intense.

During pregnancy the placenta has a powerful protective influence on the ovary. Neither at birth nor in the puerperium does the influence of the uterus come into the question, since the milk-secretion is established whether the uterus is present or not.

The secretion of milk cannot be brought about by nervous stimuli.

The secretion of milk is not produced by suckling, the latter act only promotes and maintains an already existing secretion.

The act of labour, as such, is not to be regarded as the cause of milk secretion.

Puerperal involution is nothing but a true atrophy—a physiological process consequent upon the shedding of the placenta.

CUTHBERT LOCKYER.

A Case of Pneumonia acquired in Utero.

BOCHENSKI and GRÖBEL. *Monats. für Geburts. und Gynäkol.* Bd. xxii., Ht. 4.

VARIOUS observers have shown that poisonous substances can pass through the placenta from mother to fœtus. Among chemical substances, phosphorus, mercury, and lead have been found in various parts of the fœtus, though morphia has been taken in large quantities by the mother, and yet no ill effect in the fœtus has been observed from it. Toxins and antitoxins, too, in like manner can affect the fœtus through the placenta, for instance infants have been

found to be immune to vaccination when the mother has been vaccinated during pregnancy. In maternal enteric the fœtal blood has been found to give the typical agglutination reaction. The most important question however in this regard is the passage of actual living bacteria from mother to fœtus, and although this is undoubted, the means by which they pass through the placenta is still *sub judice*. Some observers, such as Birch-Hirschfeld, Baumgarten and Lubarsch maintain that this is possible with a healthy placenta, while others, among whom are Wolff, Wyscokowitsch, and Malvoz are of opinion that some pathological lesion of the placenta is necessary. The last observer suggests that a chorionic hæmorrhage is necessary, so that maternal and fœtal blood actually mix in the intervillous space. Whichever opinion is correct we are not in a position to say, but there is no doubt that in cases of anthrax, erysipelas, small pox, measles, scarlatina, and other diseases of the mother, the specific lesions and bacteria have been definitely found in the fœtus.

The present case was a woman aged 39, who had had five children, and who was admitted to hospital in the last month of pregnancy with definite pneumonia of the lower lobe of the right lung. Three days later she was delivered of a living child. This child was cyanotic and dyspnœic when born, and showed signs of pneumonia on auscultation, *i.e.*, fine crepitations, bronchial breathing, and dulness on percussion. The child died eleven hours after birth. The *post-mortem* examination showed that it had lobular pneumonia of both lungs except at the apices, acute bronchial catarrh, and hyperæmia and parenchymatous degeneration of the liver and kidneys. Micro-organisms were found in the lungs which, in appearance, culture and inoculation into animals, were identical with the diplococcus of Fränkel-Weichselbaum. The microscopic examination of the lungs completely verified the naked-eye diagnosis of lobular pneumonia. The short period which elapsed from the birth of the child until its death, namely eleven hours, appears to exclude the possibility of post-natal infection, as the microscopic evidence went to show that the pneumonia had existed at least two days. There was no evidence in this case to show how the bacteria found their way from the mother to the fœtus, as the placenta seemed quite normal to the naked eye, and was not examined microscopically. It might be suggested that during the uterine contractions of delivery the villi were sufficiently injured to allow bacteria to pass, which would not otherwise do so. In this case, however, labour only lasted 6 hours, which added to the 11 hours extra-uterine life of the fœtus, still gives too short a period for the lesions to develope, unless infection had occurred before labour began.

THOS. G. STEVENS.

The Care of the Infant.

Practitioner, October, 1905.

THE *Practitioner* devotes its October number entirely to the consideration of the diseases of infancy, in a series of articles contributed by sixteen authors.

Dr. J. W. Ballantyne writes on the hygiene of the mother before the birth of her child. He has made this subject his own, but even he has to admit that the treatment of the unborn infant must be empiric, until such times as the mode of action of antenatal influences shall have been determined. To elucidate this he suggests routine chemical as well as microscopic examination of *all* placentæ and of the liquor amnii. He records a case in which a woman who had given birth to two male fœtuses affected with hæmophilia, was treated with calcium chloride, and bore a male child with no traces of the disease; how long this immunity lasted is not stated. He very rightly gives a prominent place to the careful testing for albuminuria throughout pregnancy.

Dr. Handfield-Jones treats of mother's milk, and makes various suggestions for cases in which the child does not thrive on it. The addition of cream to the infant's dietary for constipation, alkalies and aromatics for griping and purging are recommended. Irregular intervals of suckling change the composition of the milk; alcohol in the mother's diet increases the caseinogen and the fat in the milk. He mentions that there is an agency in Paris for supplying wet nurses drawn from the peasant population of the North of France.

Dr. Edmund Cautley discusses the artificial feeding of infants; he strongly condemns the addition by the dairyman of any preservative or colouring matter to cow's milk, or to his sterilising it by heat. Milk should be delivered clean and pure, and the customer should set it aside in a cool place, storing it in a tall jar, so arranged that the upper, richer, cleaner half can be drawn off. He maintains that more children suffer from overfeeding than from insufficient quantity of food. He gives useful tables of quantities and varieties of foods, dwelling on the importance of variety in even an infant's dietary. "Barley water has little, if any, more effect on the curdling of milk than a like quantity of alkaline water."

Dr. Still, who deals with the use and abuse of condensed milk and patent foods, condemns all starch, even so small an amount as is present in barley water, for infants under eight months. He describes several of the best known foods, and specially warns against the

assumption that a particular food is suitable because the child seems for a time to thrive on it; all patent foods are deficient in fat, and rickets is liable to follow their use, as also scurvy, if only a small quantity of fresh milk be added. Artificially added fats do not emulsify like milk and are less digestible, if even a large proportion be not lost adhering to the sides of the feeding bottle. There are useful tables of the more commonly used foods classified according to the starch they contain as such or converted.

Infantile mortality is discussed under various aspects, especially with reference to the work being done in France, of which notice has been taken from time to time in these columns. Thus Dr. McCleary has an article on the municipal feeding of infants, and insists that the work of the municipal depot must be something very much more than the mere handing of sterilised milk over a counter; it must be an organisation for the regular systematic medical supervision of the infants. We must follow on the lines of our French colleagues, whose work is described by Dr. Robinson under the title "Consultations for Infants in France." The dispensaries must at all costs avoid the reproach of favouring artificial feeding, which shows 25 times more deaths than suckling. At Nancy the women are bribed to suckle; at Rouen nursing mothers are given meat.

Dr. Newsholme studies infantile mortality from the statistical standpoint, but starts with a curious lapsus calami which makes the English mortality 859 per mil. in the first year of life. The rate in the last five years was 141 per mil. Dr. N. thinks that 75 is an ideal rate to be aimed at; among the "upper classes" it is not higher. Proper teaching will do more than crèches or indiscriminate distribution of milk.

Special diseases of infants and of the period of dentition fill up the remaining half of a very useful and interesting number.

E. H. L. Oliphant.

GYNÆCOLOGY.

On Struma Ovarii.

Ulesco-Stroganowa. *Monats. für Geburts. und Gynäkol.* Bd. xxii., Ht. 4.

This condition, or as it has been called by Meyer, struma ovarii colloides, both of which terms are quite untranslateable into English, has been considerably discussed on the continent, and signifies a new

growth of the ovary which resembles the ordinary thyroid adenoma. The author very carefully describes the microscopic appearances of a tumour the size of a fœtal head removed from a patient who was 54 years old. The tumour represented the enlarged right ovary, and was accompanied by a somewhat similar, though much smaller enlargement of the left ovary, with a large quantity of free ascitic fluid. To sum up the microscopic appearances one may say that the tumour consisted of an adenomatous structure consisting of spaces lined by low cubical epithelium, containing quite typical colloid contents, and having very little interglandular stroma. Much of this stroma had undergone hyaline degeneration, so that the gland spaces seemed in parts to touch one another, and in other places to be widely separated by a structureless hyaline medium. So characteristic were all these changes that, sections of the ovarian tumour and sections of a thyroid adenoma placed side by side under the microscope, could not be distinguished from one another. So striking was this that the author contends that these structures really were thyroid elements forming a new growth in the ovary. The origin of such a tumour has been much debated, and two rival views have been brought forward: one that these tumours are derived from the follicle epithelium, and the other that the new growth is really a teratoma in which some impulse has caused such a growth of thyroid elements as to suppress all the other tissues commonly found in teratomata. The balance of evidence is entirely in favour of the latter view, and the author holds that it is the correct one. These tumours are commonly held to be malignant by those observers who have recorded them, on the grounds that they have all occurred in women between 45 and 55, have been accompanied by much emaciation, and always associated with ascites. Also in accord with this view is Kretschmars' case, which died of recurrence of the growth two years after the operation for its removal. No *post-mortem*, however, was held on this case, and although the author's case was alive at the time of writing, too short a time had elapsed since the operation to make any definite statement as to complete cure.

The further history of this case will be looked for with much interest, and it is to be hoped that the progress will be reported.

THOS. G. STEVENS.

Tuberculosis of Cysts of the Ovary.

POLLOSSON and VIOLET. *Annales de Gynécol. et d'Obstét.*, Oct., 1905.

THE authors report two cases under their care in which an ovarian cyst was invaded by tubercle. The first occurred in a nulliparous

patient, aged 29, who had no other tuberculous lesions. She was admitted for abdominal pain of which she had complained for about two months. On examination a mass, the size of a fœtal head was discovered behind and to the right of the uterus. Abdominal section was performed, and an inflamed cyst removed, together with the right tube. The cyst was adherent to surrounding structures, and there was also an abscess deep in the pelvis. Vaginal drainage was used. Recovery was delayed by the formation of another pelvic abscess. Lining the interior of the cyst were numerous granulations, which on microscopical examination contained glandular *culs-de-sac*, while in the connective tissue of the cyst wall were grouped most characteristic tuberculous nodules undergoing caseation. The tube was stretched over the cyst, and the abdominal ostium, though congested, was not closed. Examination of the fluid removed from the cyst was negative as regards Koch's bacillus.

The second case occurred in a woman aged 32, who was also admitted on account of abdominal pain. She had been married 12 years, but had never been pregnant. The history dated back four years. On examination an elastic tumour was found reaching up to the umbilicus, part of which could also be felt in Douglas's pouch. The temperature was raised and of a hectic type. Abdominal section was performed, and a large intra-ligamentary cyst of the left ovary was removed with the tube. There were dense adhesions, and evidences of chronic peritonitis. The cyst was the size of an adult head, unilocular and with thick walls. Below the tumour was a very curious extra-cystic mass of cauliflower-like vegetations. The left tube was thickened and contained pus. Examination of the ovary and tube of the right side proved them to be affected with tubercle. The extra-cystic cauliflower-like mass connected with the cyst of the left ovary contained, on microscopical examination, numerous tuberculous foci undergoing caseation.

The question arises in such cases as to whether such cysts are tuberculous in *origin* or are they true ovarian cysts subsequently infected. Further, such conditions as encysted tuberculous peritonitis, tuberculous pyosalpinx, and abscess of the ovary, may lead to an error in diagnosis. Great care must always be taken to determine the anatomical relation of such a cyst to the Fallopian tube, as distended tubes are sometimes taken to be ovarian cysts, and *vice versa*. Similarly, care must be exercised in the microscopical examination of supposed ovarian cyst walls to determine their origin, and all suspected tuberculous areas therein contained should be submitted to vigorous criticism.

The literature of the subject is fully considered by Remy (Thèse de Lyon, 1904).

As regards clinical history, perhaps there is little to go on beyond the abdominal pain and the presence of a tumour. An acute onset of inflammation of an ovarian cyst known to have existed previously may sometimes be put down to tuberculous infection. Family history or other tuberculous lesions may help in diagnosis. Tubercle apparently attacks dermoid, as well as the ordinary glandular cysts of the ovary. It is uncommon in multilocular cysts to get all the loculi affected.

Turning to the pathology of so-called tuberculous cysts it is probable, according to the author, that most cases are really due to the infection by tubercle of an already existing ovarian cyst, and it is highly improbable that tubercle can give rise to an ovarian cyst *de novo* by formative irritation. Granting then the infection of a pre-existing cyst, must we admit such invasion as congenital or acquired? The latter is more probable, either by blood, intestino-peritoneal, or ascending infection. Peritoneal infection of a cyst may result in some cases by repeated tapping—*i.e.*, where the whole case is taken to be one of chronic tuberculous peritonitis with effusion. Again, it is reasonable to believe that a common route of infection is from a tuberculous tube or ovary. When the tuberculosis appears isolated and the ovarian lesion apparently the primary and only focus, the author believes the route of infection to be by the blood stream.

C. Hubert Roberts.

Sarcoma of the Uterus.

Piquand (G.). *Rev. de Gynécol. et de Chirur. Abdom.*, July–August, and September–October, 1905.

In the August number of this Journal there was an abstract of the first part of Piquand's long and elaborate paper on sarcoma of the uterus. It dealt with the etiology, and with the pathological anatomy of sarcoma of the body. In these two further parts Piquand discusses the pathological anatomy of sarcoma of the cervix, the histological origin of the new growths, and finally, the symptoms, diagnosis, and treatment.

The grape-like sarcoma of the cervix is very fully dealt with. Twenty-nine cases are shortly recorded. Piquand holds that these sarcomata present no distinct histological characters, but that they are simply sarcomatous polypi which have undergone an œdematous

degeneration, and which differ from other sarcomata of the uterus only in their macroscopical appearances. This latter he attributes to two factors, their origin in the mucous membrane of the cervical canal, rich in papillæ, and their growth into the vagina, which unlike the body of the uterus, is both elastic and extensible.

Apart from the grape-like form Piquand has collected forty-one examples of sarcoma of the cervix. The new growth usually arises in the mucous membrane, and is of the circumscribed form, either sessile or polypoid. As regards their histology there is nothing particular to note, save the frequent presence of glandular elements, striated fibres, cartilage, etc. This is true of the grape-like sarcomata as well as of the more ordinary forms.

The discussion of the cellular origin of sarcomata is divided into two sections:—(1) new growths of the parenchyma, (2) of the mucosa.

I. Piquand begins by insisting on the sarcomatous degeneration of fibroids, not as a rare but as a frequent occurrence. He uses the term "degeneration," though it seems doubtful whether it is one which can be correctly used in connection with the appearance of a cellular new growth.

He proceeds to discuss the development of the sarcomatous cells under three headings:—

(1) By proliferation of the connective intra-muscular tissue.

(2) By proliferation of the cells of the walls of the blood-vessels or lymphatics.

(3) By transformation of the muscular fibres.

In connection with this last the observations of von Kahlden, Whitridge Williams, etc., are considered at length, and the conclusion is reached that all three sources must be admitted as proved. As regards the second heading, the question might be asked whether such new growths would not be better classified as endotheliomata.

II. In the mucosa the new growth must proceed from one of two sources, either from the connective tissue, or from the walls of the vessels, and in most cases the two processes of proliferation are associated, and occur simultaneously.

Discussing the appearance of striated muscular fibres in sarcomata of the uterus, Piquand gives reasons for doubting their exclusive origin from aberrant embryonic germs. He states that the existence of transition forms between unstriated and striated fibres has been clearly established, and he is of opinion that the striated fibres owe their presence in the majority of cases to a transformation or metaplasia of the unstriated muscular fibres.

The occasional occurrence of sarcoma and carcinoma in the same uterus is well known. Cases of sarcoma of the body with carcinoma of the cervix, and on the other hand Klein's case of a sarcomatous polypus coinciding with cancer of the body, may be regarded as simple coincidences, but there are other rarer cases where the sarcomatous and carcinomatous tissues are associated in one and the same tumour. Piquand gives the names of seven authors as responsible for these observations.

The clinical aspect of these uterine sarcomata is as fully considered in the October number of the *Revue* as the pathological in the two previous parts. In the case of the parenchymatous sarcomata of the body the difficulty is to distinguish them in their early stages from fibro-myomata, especially when the latter are the subject of œdematous degeneration. When the rapid growth is associated with cachexia, œdema, ascites, or dilatation of the veins of the abdominal wall, the distinction may be made, but in many cases it is impossible before opening the abdomen.

The prognosis in all cases of sarcoma is extremely grave. Rapid recurrence after operation is only too common. Piquand thinks that there is a slightly better prognosis in sarcomata arising in the mucosa than in the case of the parenchymatous growths. In the latter a good deal depends on the histological characters, the pure round-celled form being the most dangerous. In the former, on the other hand, it is not so much the characters of the cells as the direction of growth; those invading the uterine wall being much less favourable than those which tend to form polypoid masses in the uterus and vagina.

The diagnosis of many of the sarcomata of the cervix is by no means easy. It often happens that a polypoid growth is removed, and it is only its recurrence which leads to suspicion and a histological examination. The figures given by Piquand would lead to the impression that the prognosis in sarcoma of the cervix is even more grave than in sarcoma of the body, but Piquand himself warns us against this conclusion, and expresses the view that with *early* hysterectomy in all cases the prognosis would be more favourable in the cervical growths.

With regard to treatment there is now general agreement that the uterus must be completely removed wherever possible. In certain cases disagreement may occur as to whether the abdominal or vaginal is the preferable route. For the discussion of this point, and for the detailed description of the symptoms, and also for the differential diagnosis, reference must be made to the original paper, which in its

three parts, covering altogether 140 pages of the *Revue*, is an exhaustive study of the subject. It is worthy of remark that it should have appeared in the same year as Piquand's book on "The Degeneration of the Fibro-myomata of the Uterus," which was reviewed in the September number of this JOURNAL.

R. HAMILTON BELL.

Abdominal Hysterectomy for Uterine Cancer.

POLLOSSON (AUGUSTE). *Rev. de Gynécol. et de Chirur. Abd.*, September-October, 1905.

BETWEEN November, 1904, and July, 1905, the author operated 32 times for cancer of the uterus, cervix and body, by the abdominal route, following very closely the technique of Wertheim. Only the immediate results can, of course, be given. Before entering into the details of his cases Pollosson discusses the merits of the vaginal and abdominal routes respectively. Up to November, 1904, his own practice had been to operate by the vaginal route, save in cases too far advanced, when he had sometimes attempted abdominal extirpation, without, however, systematic dissection of the ureters, or search for infected glands. It is not surprising that these advanced cases yielded him an increased operation mortality, and that in those that survived there was rapid recurrence. The vaginal hysterectomies, too, gave him little encouragement. He was led to progressively diminish the number of cases submitted to operation, until his figures were 12 to 14 for 100 observed cases, and even then "recurrence was nearly constant, permanent recovery the exception."

Pollosson quotes the figures of other French surgeons relating to vaginal hysterectomy. Bouilly, in 1897, reported 77 per cent. of recurrences in the first year, and could only be sure of one case without recurrence, and in this the diagnosis was doubtful. Segond, in 49 cancers of the cervix, the after history of which was known, reported 40 recurrences and 9 recoveries, but of these nine 2 only had passed five years from the date of the operation, 2 were four years and 5 two years. Very few of the French surgeons give their figure of operability. Pollosson believes that it is in general very low. Pozzi, in 1902, quoted 46 vaginal hysterectomies performed in twelve years, less than four a year, which must correspond to a very low figure of operability. Duret, in 1904, said that out of 173 cases he had operated on 69, but taking cancer of the cervix only, his percentage of operability was about 20.

Pollosson contrasts the French figures with those of the German surgeons, which show such a striking superiority. Taking no recurrence for five years as a recovery, Schauta has 26·4 per cent., Olshausen 30 per cent., Pfannensteil 45 per cent., Leopold 50 per cent. The figure of operability for these surgeons has steadily increased, in the case of Olshausen from 31·6 per cent. to 61·6 per cent., Schauta from 14·7 per cent. to 46·5 per cent., Pfannenstiel from 28·6 per cent. to 53·6 per cent. Leopold's figure of operability is given as 20·4 per cent. To sum up, the German surgeons report a much higher figure of operability, and, notwithstanding this, a much better percentage of permanent recoveries.

Pollosson thinks that two main factors account for this marked difference—firstly, the energetic campaign against cancer has led to the German surgeons seeing the cases early; and, secondly, the adoption of the liberating incisions of Schuchardt. But though these comparatively good results were obtained in Germany by the vaginal route, many surgeons there were not satisfied, and Küstner, v. Rosthorn, Wertheim, etc., persistently advocated the abdominal method. The question to be asked is, what are the advantages of the abdominal route? And the first point to be considered in reply is, the extension of operability. The average of operability by the vaginal route in Germany is 45 in 100, by the abdominal route it reaches 60 to 70 in 100. Pollosson himself had a figure of 12 to 14 in 100, while since November, 1904, the date of his adoption of the abdominal route, his percentage of operability has risen to 56.

As to mortality, Pollosson claims that it is steadily diminishing, and is now somewhere about 15 to 18 per cent., while in the hands of some surgeons it is considerably less. It must also be remembered that the mortality must not be compared with the figures obtained by vaginal hysterectomy in favourable cases, that is, when the surgeon has a low figure of operability, and does not attempt the removal of extensive growths.

As to recurrences, perhaps the most important point, it is hardly possible yet to speak with certainty, but Wertheim and Mackenrodt's figures, given at the Breslau Congress in 1904, were distinctly favourable to the abdominal route.

It can scarcely be doubted that, apart from the removal of infected glands, a wider margin can be obtained by the abdominal than by the vaginal route, and Pollosson states, from his own experience, that this margin progressively increases as the surgeon becomes better acquainted with the technique. Other advantages claimed by Pollosson for the abdominal operation are (1) greater

facilities for dealing with wounds of the bladder, rectum or ureter; (2) greater security of asepsis; (3) less risk of grafting cancer during the course of operation; and (4) better facility for dealing with complications, such as salpingitis.

Pollosson next proceeds to discuss the indications and contra-indications for the operation. The age and general condition of the patient are very important considerations. Profound anæmia and cachexia, the presence of albumen, or of œdema of the legs are contra-indications. Obesity adds greatly to the difficulties of the operation. Involvement of the parametrium and the ureter, does not in itself afford a contra-indication, nor does invasion of the bladder or anterior wall of the rectum, so long as the mucous membrane is not involved. These points are discussed at length, and this short summary conveys a very imperfect impression, even of the conclusions reached. The reader must be referred to the original article, as also for a description of the technique Pollosson employs, which in its essentials is that of Wertheim.

It remains to refer to the analysis of the 32 operations. As the cases are reported so soon after operation, less than twelve months from the first of the series, the very important point of freedom or otherwise from recurrence cannot be considered; but it is of interest to see what are the immediate results obtained by a surgeon in his early efforts to carry out what is undoubtedly a difficult and trying procedure. During the eight months (November, 1904, to July, 1905) Pollosson saw 55 cases of cancer of the uterus. Of these 5 were cancers of the body, of which 4 were operable, and 50 cancers of the cervix, of which 27 were submitted to operation (the total of 32 operations is made up by recurrence in one case for which the abdomen was re-opened). The figure of operability for cancer of the cervix is therefore 54 for 100 cases. Pollosson states that four of these would certainly have been operable by the vaginal route, and two others possibly so. Taking the highest figure (six), it gives an operability of 12 only for 100 cases. Six deaths occurred in the 32 operations. One of these was in a case of carcinoma of the body, leaving 5 deaths in 27 operations for carcinoma of the cervix, or an immediate mortality of 18·5 per cent. In two cases death occurred from shock, respectively half an hour and less than 24 hours after operation. Both patients were over fifty, and both were very anæmic. Another patient succumbed two months after the operation, as the result of a phlebitis of both lower limbs. The rectum was injured at the operation, and sutured immediately, but the suturing failed and a recto-vaginal fistula resulted. The wound thus

became infected, and the fatal phlebitis supervened. In another case death occurred eighteen days after the operation, peritonitis developing secondarily from a primary suppuration in the abdominal wall. The fifth case died suddenly on the twelfth day, upon getting up, after an absolutely normal convalescence up to that point. At the *autopsy* there was no trace of embolism or cerebral hæmorrhage, but extreme congestion of the meninges and of the kidneys. Pollosson considers this death as independent of the operation, and excludes it when himself calculating his mortality, which he gives as 5 cases in 32=15·6 per cent. Besides the wound of the rectum mentioned above, which led to a fatal result, the bladder was wounded twice and the ureter once. The wound of the bladder was not noticed in one case, and a fistula resulted, but it closed spontaneously. The divided ureter was implanted into the bladder, but a vaginal fistula developed late in convalescence. The patient was re-admitted later and the fistula successfully closed. Pollosson has had no case of necrosis of the ureter, though a careful dissection of both ureters was made in 24 of the 27 cases of cancer of the cervix.

Summing up his own impressions, the author gives these figures: 5 operations unsatisfactory; 5 doubtful, on account of the extent of the new growth; 15 satisfactory; 6 deaths; total 31.

R. Hamilton Bell.

Abdominal Hysterectomy for Cancer of the Cervix complicating Pregnancy.

Pollosson (A.). *Annales de Gynécol. et d'Obstét.*, August, 1905.

Having had occasion to perform abdominal hysterectomy for cancer of the cervix in a pregnant woman, the author enters into a discussion as to the comparative value of the abdominal *versus* the vaginal route in such cases. Probably the question is still *sub judice*, but the trend of modern operators is, or seems to be, in favour of the abdominal method. Condamin (R. and A.), Sarwey, Orthmann, Carstens, Glockner, and others, have contributed much to the later literature on this interesting subject. Perhaps up to 1899 most authorities agreed in the main on certain points, and O. Sarwey (in *Veit's Handbuch*) sums up the matter thus:—During the first four months total vaginal hysterectomy, as soon as the disease is recognised, is the best proceeding; during the 5th, 6th, and 7th months other operations may be performed, viz.:—

(*a*) Total abdominal hysterectomy.

(*b*) Combined abdominal and vaginal hysterectomy, removing the body of the uterus by the abdomen, and the cervical mass by the vaginal route.

(*c*) A third group of operators advise that complete vaginal hysterectomy *immediately* following an abortion either spontaneous or induced.

Pollosson puts aside completely the question of inducing abortion first, and then *some time after* performing vaginal hysterectomy.

During the 8th, 9th, and 10th months (lunar) the fœtus is viable, and both the fœtal and maternal lives enter into consideration, hence vaginal or abdominal Cæsarean section should be performed, followed by removal of the uterus by the abdomen, or vagina, or both.

Inoperable cancer, of course, is not considered, since then the life of the fœtus becomes predominant.

Latterly vaginal hysterectomy has been most often performed, and certainly with success, and even up to this present year R. and A. Condamin, in their recent article on cancer and pregnancy (*Annales de Gynécol.*, March, 1905), are strong supporters of the vaginal route. Pollosson grants that *published* cases of vaginal hysterectomy are more numerous, and therefore seem to give the better operative results. On the other hand, cases of abdominal hysterectomy in pregnancy are few and far between, and those cases that are published at present do not compare favourably with the vaginal method, so that in mere documentary evidence vaginal hysterectomy would seem to gain the day. Taking, on the other hand, the results of abdominal hysterectomy for cancer apart from pregnancy, Pollosson maintains that every day the abdominal route is gaining favour. This method is far more complete, the limits of the disease can be determined, extensive removal of infected parametrium and glands is possible, and if the vagina is closed (from above) by clamps (after cauterisation of the growth) before section (as in Wertheim's operation), the question of sepsis and local cell infection are diminished or completely abolished. The author maintains that all these arguments apply equally to cancer complicating pregnancy. Further, that as our operative technique advances as it has done in recent years, many of supposed risks of the abdominal route will disappear. In the past it was only the most unfavourable cases that were set aside for the abdominal operation, while apparently favourable cases were attacked per vaginam. Hence the published statistics of the abdominal route *versus* the vaginal perhaps compare unfavourably. With regard to combined operations consisting of supra-vaginal amputation by the abdomen and consecutive extirpation of

the cervical mass by the vagina these seem rather to be allied to vaginal interventions than to abdominal hysterectomy performed in the modern technique.

Against vaginal extirpation of the cancerous cervix it is urged that any liberation of the ureters is impossible, and for this reason the operator is quite unable to remove any infected parametrium which in itself constitutes the chief advantage of the abdominal route.

C. Hubert Roberts.

The Value of Removing the Glands in the Operation for Cancer of the Uterus.

Baisch. *Archiv für Gynäkol.* Bd. lxxv., Ht. 2.

The material on which the author bases his results was obtained from 125 cases of cancer of the uterus in Döderlein's clinic. Serial sections were made of lymphatic glands obtained at operation, and in the *post-mortem* room, the latter including eighteen cases which died shortly after operation. Corporeal and cervical carcinoma are sharply differentiated. Baisch found in twenty-four cases of corporeal cancer only four had carcinomatous deposits in the glands, and thinks, as do others, that total vaginal hysterectomy is sufficient for cancer of the body. On the other hand in cervical cancer 50 per cent. of the cases, or even more, showed parametric infiltration. Baisch therefore concurs with Kundrat in advising the removal of the parametric tissues invariably in carcinoma cervicis. With regard to the lymphatic vessels and channels Kundrat and Œhlecker have proved that they are always free so that the task of trying to remove the entire lymphatic network is superfluous. Upon the question of the frequency with which the glands are diseased opinions differ widely. In the series compiled from the work of Döderlein, Schauta, and Œhlecker (Olshausen) thirty-five cases showed eleven, or 31 per cent. of glandular involvement. We can strike an average and say that one-third of all operable cases of cancer of the cervix have deposits in the glands. For radical treatment of such cases of course the vaginal route must be renounced. Baisch further noted that in 72 cases in which the parametrium appeared clinically free from disease, in 12, *i.e.* 16 per cent., there was glandular involvement, whilst in cases where the parametrium was diseased malignant glands were found in 50 per cent. This means that clinically normal parametric tissues does not mean freedom from disease in the glands, but in such cases we must reckon on $^1/_{10}$ to $^1/_5$ as having cancerous glands. Baisch

and Pfannenstiel both agree that there is no risk of glandular deposit in the early stages of portio-carcinoma. In 4 cases of early cancer of the portio no cancer was found in serial sections made of the glands. In 12 cases of cancer of the vaginal cervix, 5 showed cancerous glands. Therefore for early cancer of the portio Schuchardt's operation would be suitable.

The histological features must not, however, be taken as a universal guide in determining whether to choose the abdominal or the vaginal route in operating.

Wertheim's experience teaches that in cases where the glands have to be removed for cancer recurrence is to be expected, as we are not in a position to be able to discover all the affected glands. The author does not advance this as an argument against the abdominal operation, but says there is always a possibility of actually removing all infected glands and further pleads for this method that the wide removal of the parametric tissues must not be undervalued.

CUTHBERT LOCKYER.

What Methods of Ventral Fixation lead to Trouble in Labour and to Ileus?

SEEGERT (PAUL). *Zeits. für Geburts. und Gynäkol.* Bd. lv.

SEEGERT has looked up the records of cases in which labour has followed ventral fixation by Olshausen's method, *i.e.*, suturing the proximal parts of the round ligaments, together with part of the broad ligaments, to the abdominal wall. He considers that these records prove that this method is the one least likely to be followed by trouble in labour. Certain accidents, *e.g.*, suppuration of the abdominal wound, and exudation from the stump left after removal of diseased appendages may cause trouble in cases operated on by this method, as well as in those operated on by any other method, by making the adhesions too dense and unyielding. Apart from these accidents the adhesions formed are strong enough to keep the uterus from falling back, but not too dense to allow of the uterus rising up in pregnancy.

He has collected four cases of intestinal obstruction following ventral fixation.

Jacobs reports one fatal case after direct fixation of the anterior wall of the uterus to the anterior abdominal wall. The adhesion was $4\frac{1}{2}$ c.m. broad. Olshausen has recorded a case after direct fixation by Leopold's method.

Dardanelli reports a case in which adhesions between tube and intestine were noticed at the time of the operation, and were not separated.

In the fourth case death occurred one month after fixation of the fundus with 1 catgut and 2 silk-worm-gut sutures.

No case of intestinal obstruction has been recorded after employment of Olshausen's method.

HENRY RUSSELL ANDREWS.

Combined Malignant Tumours of the Female Genitalia.

FINDLEY (PALMER). *Surgery, Gynæcology and Obstetrics*, Oct., 1905.

THE author reports the following case:—

The patient, a vii.-para, æt. 47, suffered from a small polypus, which protruded from the external os. This was removed, and found to be a mucous polypus which had undergone carcinomatous degeneration. Hysterectomy was done a few days later, and the patient made a good recovery. A year later a polypoid growth was found protruding into the vagina from the base of the left broad ligament. This was excised, and in four days it had returned to twice its original size of an almond. It was found to be a mixed-cell sarcoma. Two plates are given showing the carcinomatous degeneration of the original polypus, and the mixed-cell sarcoma.

Twelve somewhat similar cases are referred to from German literature.

ROBERT JARDINE.

Tubal Twin-Pregnancy with Two Ova of Different Sizes.

SANITER (ROBERT). *Zeits. für Geburts. und Gynäkol.* Bd. lv.

THE author's patient had a slight fainting attack, followed by abdominal pain, 5 weeks after her last menstrual period. She was examined a few days later, and thought to have an early intra-uterine pregnancy, with threatened abortion. As the bleeding continued, Saniter had her anæsthetised eleven days later with the idea of making a thorough examination and probably emptying the uterus. A doubtful thickening in the region of the right appendages was felt, and the uterus was curetted, a few small fragments of decidua being removed. A few hours later, another fainting attack occurred, three weeks after the first. The abdomen was opened and the right appendages were

removed. Together with some adherent blood-clot they formed a mass about the size of a fist. The abdominal cavity contained free blood. Examination of the right tube showed that it contained two ova, of different sizes, the smaller being situated 2–3 c.m. from the uterus, the larger near the fimbriated extremity, separated from the smaller by 2 c.m. of tube. The smaller ovum had a diameter of 1–2 c.m., the larger, which was about the size of a hen's egg, contained a fœtus 4 c.m. long. Both ova had caused ruptures, the smaller one only a punctiform tear, the larger a gaping opening in which the ovum rested as if in an egg-cup.

There was no need to consider the question of superfœtation: probably the ovum nearer to the uterus had ruptured the tube at the time of the first fainting attack, and the larger one had gone on developing for another three weeks. An elder sister of the grandmother of the patient had also had twin pregnancies.

Tubal pregnancies complicated by twins may be divided into three classes. In the first, much the most commonly met with, one ovum is extra-uterine, the other intra-uterine. In the second, the least common, each tube contains an ovum. In the third, both ova are in one tube. Saniter's case brings the number of recorded cases of the third class up to 20.

HENRY RUSSELL ANDREWS.

Repeated Extra-uterine Gestation in the same Fallopian Tube Twin Pregnancy.

MICHIN. *Monats. für Geburts. und Gynäkol.* Bd. xxii., Ht. 4.

THE manifestations of extra-uterine pregnancy are now known to be so varied and so many have been described that no surprise need be felt at one or more additions. The case to be described is interesting as bearing upon the question of treatment, and upholds those authorities who consider that complete extirpation of the gestation sac is the proper treatment in all cases. The author is of this view, and holds that vaginal incision and clearing out the sac is contra-indicated for two reasons:—(1) That many small portions of the sac remain in the pelvis which may undergo various inflammatory processes; and (2) that no one can ever be quite sure after such an operation, that the pathological pregnancy will not be repeated in the same place. The author's case bears out this objection very clearly.

The patient was 32, had been married 8 years, and had had no children. Two years previously she had been treated in a provincial

hospital for a ruptured left tubal gestation. The operation consisted in clearing out the contents of the gestation sac through a vaginal incision and speedy recovery took place. After this menstruation was regular, but profuse and painful. The last menstrual period occurred on October 5th, 1904, and early in December the patient complained of pain in the pelvic region and had bleeding from the vagina. A swelling was found to the left of the uterus as large as a fist, whilst the right ovary was enlarged and cystic. The diagnosis of left tubal gestation with internal bleeding was diagnosed, and consequently the abdomen was opened. The diagnosis proved to be correct, and both the right ovary and left tubal gestation sac were removed. The latter showed two distinct cavities, one which was evidently old and contained remnants of old blood-clots, and another which contained fresh blood-clot in which two fœtuses were found. The left ovary was removed with the gestation sac, and contained a true recent corpus luteum of pregnancy. The patient made a complete recovery.

THOS. G. STEVENS.

Teratoma of the Labium Majus (Intestinal inclusion).

DUCLAUX and HERRENSCHMIDT. *Bulletin et Mém. de la Soc. Anat. de Paris*, May, 1905.

THE authors of this paper state that they have not been able to find any record of a case similar to the one here described. The occurrence of structures of the nature of the large bowel at the anterior limits of the perineum is an extremely rare event. The specimen is therefore described in considerable detail, and three figures are given to illustrate its naked eye and microscopic features.

The patient from whom it was obtained was admitted to hospital for a congenital tumour of the right labium majus. She had noticed no increase in its size, nor had it given rise to any pain, and she had sought advice solely because of the discomfort it had caused. This was especially marked on walking, as the friction had given rise to some irritation of its surface with slight muco-purulent discharge. The tumour was found to be about the size of a goose's egg; it was soft and semi-fluctuating, and occupied the length of the labium. It was smooth on the surface, but presented two small nipple-like elevations—one at its upper margin and the other at its external border near the genito-crural fold. On the free edge of the labium was an opening leading into a diverticulum of 5 to 6 cm. in length;

it was very similar in appearance to a colotomy wound, and exuded a muco-purulent fluid. The patient was found to have also a double uterus and vagina.

The tumour was excised and submitted to a careful pathological examination. On section it was found to be made up mainly of fatty tissue. The diverticulum, on being opened up, showed a reddish-blue surface thrown into circular folds. The upper nipple-like projection contained a small cystic cavity about the size of a small nut filled with a greenish fluid, the other was made up entirely of fat and connective-tissue. Histological examination of the mucous lining of the diverticulum showed that it corresponded to the mucosa of the large intestine, and especially to that of the rectum. The surface epithelium was composed of high cylindrical cells; villi and agminated follicles were entirely absent, but glandular invaginations exactly similar to the crypts of Lieberkühn were present in large numbers. Filling up the inter-glandular spaces were masses of lymphoid tissue. In the submucosa were groups of glands which recalled those of the mucous salivary glands. Outside this was a muscular tunic, composed of two layers of unstriated muscle fibres running in directions perpendicular the one to the other. The structure of the small cyst in the nipple-like projection also indicated its origin from the mucosa of the large intestine.

The authors consider that the existence of a normal anus in this patient, as well as of a well-developed perineum, puts the condition outside the commonly described malformations of the anus; it is not a condition of arrested development, but a fœtal inclusion of one or more portions of the lower gut in the labial tissues. The fact that it was accompanied by a malformation of the genital canal shows that there must have been some considerable embryonic developmental disturbance since structures derived from both endoderm and mesoderm were involved.

J. S. Fairbairn.

REPORTS OF SOCIETIES.

OBSTETRICAL SOCIETY OF LONDON.

Wednesday, November 1st, 1905, A. C. BUTLER-SMYTHE, F.R.C.S.EDIN., *Vice-President, in the Chair.*

SUBTOTAL HYSTERECTOMY: AFTER-HISTORIES OF SIXTY CASES.

Mr. ALBAN DORAN in this paper tabulated and analysed sixty after-histories of cases where he had performed subtotal hysterectomy for fibroid disease in women who had not reached the menopause, in order to determine how far that operation is liable to be followed by disagreeable consequences. In every instance a reliable after-history, extending to at least two years after operation, could be obtained, and cases with short or otherwise imperfect histories were rejected. As regards the removal of the tumour the results of subtotal hysterectomy appeared uniformly beneficial, but certain discomforts, more or less associated with artificial menopause, sometimes followed the operation, and the cases were brought forward in order to help in indicating how far these discomforts might be avoided.

The preservation of one or both ovaries was never justifiable when, as is not rare, cystic or inflammatory disease is present, but when healthy at least one ovary should be saved. Abel and Zweifel maintain that it is essential to preserve a portion of the endometrium as well, in other words, to amputate the uterus above the level of the os internum, in order to ensure the patient against the discomforts of an abrupt menopause.

In twenty-eight cases both ovaries were removed. In three the menopause was neither immediate nor complete, and in at least two the menstrual blood must have come from the endometrium. In six the menopause was complete without symptoms; all the six patients were over thirty-eight years of age. In nine the menopause was distinct, complete, and mild, whilst in ten it was complete with severe symptoms. In the worst case the patient was a woman aged thirty-eight, of doubtful sanity and intemperate habits. For three years her health remained good, but she was troubled with flushings; then she resumed her bad habits, and, at the end of six months, died after drinking carbolic acid. In another case where the patient had been insane before the operation the mental symptoms recurred a few months later, but soon subsided.

In twenty-six cases one ovary was saved. In eight the catamenia were regular for a longer or shorter period after operation. In five the catamenia appeared after operation, but irregularly. In thirteen the menopause was complete and immediate after operation, without symptoms in four patients, whilst in none of the remaining nine were the symptoms severe, as in the similar sub-series, where both ovaries were sacrificed.

In six cases both ovaries were saved. In three the catamenia continued regular after operation, in two they reappeared but were soon suppressed, and in one the menopause was complete and immediate after operation, without symptoms.

The author considered that the Abel-Zweifel theory seemed to receive support from his statistics. Thus, in seven out of the eight cases where one ovary was saved and the catamenia continued regular, and in two out

of the three where both ovaries were saved and the catamenia continued regular, he had purposely amputated the fibroid uterus above the level of the os internum. In many cases where some endometrium was spared in this manner the period became irregular or never reappeared, but, in these instances, the portion left behind might have been diseased or damaged during operation, or destroyed in the course of cicatrisation of the stump of the uterus. Flaps of uterine wall without endometrium were, according to these statistics, insufficient as guarantees against severe menopause symptoms.

A similar analysis of sixty cases of panhysterectomy for fibroid where, of necessity, no trace of endometrium can be preserved would be instructive.

Mr. Butler-Smythe congratulated Mr. Doran on the excellent work done in tracing the after-histories of his cases and hoped that other operators would follow his example.

Mrs. Boyd suggested that in appraising the effect of operation in the production of menopause symptoms, it was important to bear in mind how varying were the symptoms accompanying the natural menopause in different individuals. In twentytwo cases of supra-vaginal amputation with retention of one or both ovaries operated on up to the spring of 1904 she had investigated the after-results. In all but three the results were excellent, and in some cases striking as to the general health, and the three exceptions were in poor health before operation. In none were there really severe menopause symptoms, and in only one did a periodic sanguineous discharge persist. In that one she was not aware that she had left any endometrium, but she had noted that an inch and a half of cervix remained. In all the other cases amputation was at or below the internal os.

Dr. Herbert Spencer said he considered that the title "subtotal" hysterectomy was a horrid expression, and, moreover, was inapplicable to an operation in which the whole of the cervix and part of the body were left behind. Personally he had always endeavoured to leave one or both ovaries in every case of hysterectomy for fibroids. Dr. Spencer thought it was proved beyond question that the retention of the ovaries was of value to the patient, and was not followed immediately by flushings, whereas flushings usually occurred when the ovaries were removed. Zweifel's views had recently been disproved. He was sorry that he could not at present give the after-histories of his cases for comparison with the after-histories given in Mr. Doran's paper; but as he had in every case endeavoured to leave one or both ovaries behind he had no doubt that they would compare favourably, with regard to the occurrence of flushings, with Mr. Doran's cases in nearly half of which both ovaries had been removed. One case, however, was sufficient to disprove the statement that it was essential to preserve a portion of the endometrium. He had, two or three days ago, seen a case in which he had performed total abdominal hysterectomy seven years previously for cancer and fibroids in a woman of thirty-six, one ovary being left behind. The patient had no flushings nor other symptoms of the menopause for six years after the operation, and then the symptoms were slight. He remained of the opinion that total hysterectomy was greatly superior to the partial operation. The mortality had recently been shown to be less than that of the partial operation.

Dr. Heywood Smith remarked that in most of the vertebrata the

organs for the elimination of the sperm and germ cells respectively were duplicated, but it was not known whether this was merely to guard against disease or accident, or for some other purpose. Further investigation into the natural history of menstruation was required in order to judge the effect of the removal of one or both ovaries.

Dr. W. S. A. GRIFFITH altogether disagreed with Mr. Doran in his statement that the preservation of the ovaries in the operation of hysterectomy "is never justifiable when cystic or inflammatory disease is present," a statement which Mr. Doran would certainly not have made had he been writing from the point of view of the disease of the ovary.

Mr. MALCOLM thought that further evidence as to the variation in the severity of the menopause in women generally was desirable. The worst cases of climacteric symptoms which he had met with occurred, one in a girl of twenty-three, from whom two dermoids, very adherent from twisting of one pedicle, had been removed; the other in a woman of fifty-four, from whom a very hard calcareous uterine tumour was taken four years after the periods ceased. The effects of removal of both ovaries were extremely variable. He was not persuaded by the evidence brought forward by Mr. Doran to alter his practice of removing the uterus completely.

DIMINISHED ALKALINITY OF THE BLOOD IN ECLAMPSIA.

Dr. C. NEPEAN LONGRIDGE read a short communication on a case of Eclampsia, which appears as an original article on p. 399 of this journal.

Dr. HANDFIELD-JONES quoted two cases in which the patients had had convulsive seizures in several consecutive confinements, and in whom the kidney function had been most regularly performed in the intervals. He was much interested in Dr. Longridge's observations.

Dr. EDEN considered the observation of Dr. Longridge as to the marked diminution in the alkalinity of the blood to be of great practical importance, for if it should prove to be a constant occurrence in eclampsia, then a definite advance was assured in our knowledge of the nature of the toxæmia. By using a definitely alkaline solution for transfusion an immediate effect upon the convulsions might be confidently anticipated.

SPECIMENS

Dr. WALTER TATE showed a fibromyoma of the uterus, associated with a large cavity containing retained menses and communicating with the uterine canal, which had been removed from a woman of forty-nine. He thought that the cavity had been produced by a gradual expansion of the anterior wall of the uterus at the junction of the body and cervix, and possibly without any actual obstruction.

Dr. HANDFIELD-JONES suggested that the fibroid, by bulging into the uterine cavity, might have acted as an impediment to the outflow of the menstrual fluid.

Dr. WALTER TATE showed a degenerating fibromyoma and sarcoma in a uterus removed by operation from a single woman thirty-five years of age. He raised the question as to whether the sarcoma was due to malignant change in a pre-existing fibroid or whether it was a primary growth.

Dr. HANDFIELD-JONES showed a polypoid growth, which proved to be a pure myxoma, which he had removed from the left labium majus of a young woman aged thirty-eight.

BRITISH GYNÆCOLOGICAL SOCIETY.

At a meeting on November 9th, 1905, Dr. WILLIAM ALEXANDER, *the President, in the Chair,*

Dr. MACNAUGHTON-JONES read notes on a specimen of

EARLY TUBAL HÆMATOCOELE

in a patient aged twenty-three, whose symptoms appeared immediately after menorrhagia. During convalescence, symptoms of internal hæmorrhage supervened, necessitating immediate operation. The abdomen contained blood and clots , and the right tube was found to have a rent extending through the right broad ligament to the posterior surface of the uterus. Hæmorrhage only ceased after supra-vaginal hysterectomy. Degenerated chorionic villi were found in the dilated right tube.

Professor TAYLOR exhibited three specimens from cases of nephrectomy in women. The first was a tumour of the right kidney, involving the lower part of the anterior surface, from a patient aged fifty-three, who had had six children. The symptoms were those of distended gall bladder. On operation a tumour of the right kidney was found intimately adherent to the colon. The second tumour was one of tuberculous disease removed from a patient aged forty, with six children. The symptoms were those of cystitis. The diagnosis was made from the condition of the ureter on vaginal examination. The third was from a case of pyonephrosis where pus had been present in the urine for three years. The tumour was removed and the patient recovered. These cases were mentioned in order to discuss the value of segregation in diagnosis.

Dr. BEDFORD FENWICK showed a fibroid uterus containing a fœtus of three months removed from a patient aged forty, who had been married for seventeen years and was pregnant for the first time. The history was one of amenorrhœa with increasing pelvic pain and dysuria. The pelvis was filled by a tumour which extended to the umbilicus. Hysterectomy was performed. The tumour was particularly dense and contained a fœtus about three months old. There were numerous and firm adhesions.

Dr. WILLIAM DUNCAN showed a uterus removed by Porro-Cæsarean section for contracted pelvis. On section of the uterus the placenta was anterior, and a living child was removed through it. Sterilization by removal of the uterus was advocated in such cases rather than a more conservative procedure.

Dr. SMALLWOOD SAVAGE read a paper, illustrated by lantern slides, on *Hæmatoma of the ovary and its pathological connection with the ripening and retrogression of the Graafian follicle.* The maturation and retrogression of the normal follicle were first described. A series of cases were given where one or both ovaries had been removed for severe pain associated with tumour. The average age was twenty-eight and a half; about half were single; dysmenorrhœa occurred in most. When the onset was acute, the symptoms simulated ectopic gestation. Illness lasted for a considerable period. The hæmatoma was bilateral in half and occurred on the two sides in nearly the same proportion. Thick and vascular adhesions were found in all cases. The contents varied from a thin watery

blood-stained fluid to a viscid, tarry, chocolate-coloured mass. The cases were considered in two groups; in the one hæmatoma occurred at an early stage, and in the other at a later stage. The microscopical and macroscopical appearances were described. These hæmatomata result from multiple and premature ripening of graafian follicles, which is most probably due to inflammation of the ovary; an escape of blood takes place into them from the vessels of the theca interna; the cysts unite, and, being immature, by reason of the absence of the presiding ovum, the normal retrogression of the follicle does not take place, and a blood cyst remains. The pathological condition results more from rupture of blood vessels than from bursting of follicles.

The discussion was postponed.

REVIEWS OF RECENT BOOKS.

ESSENTIALS OF OBSTETRICS, arranged in the form of questions and answers prepared especially for students of medicine. W. E. Ashton, Professor of Gynæcology in the Medico-Chirurgical Society of Philadelphia. 6th Edition, revised. 75 illustrations. Henry Kimpton, London. Pp. 249. Price 4s.

Although this book has reached its 6th edition, it is a form of work which has never had much popularity with teachers in this country. The "question and answer" method does not form the basis of true learning, and cannot but be looked upon as savouring very much of "cram." The author states that he believes the answers in it represent the up-to-date views of obstetric masters. We must be pardoned if we do not completely agree with him in this statement. As an example, no mention is made of Peter's ovum in the development of the decidua, villi and embryo. And the old disproved view of the formation of the decidua reflexa is the only one given. Again, no mention is made of recent views on the subject of hydatidiform mole; Champetier de Ribes's bag is not mentioned in connection with placenta prævia; immediate delivery is recommended for accidental hæmorrhage whether grave or slight, and no mention is made of the Rotunda method. These examples suffice to show that in respect of these conditions, at all events, the views represented are by no means up-to-date. We cannot but believe that the student who reads this book will learn very little unless he gets the answers by heart, and then it is very questionable whether he can be considered to have acquired a sound knowledge of obstetrics.

INDEX OF SUBJECTS.

Abnormality of the introitus, 249
Abortion, tubal, 348
Adeno-myoma of the female generative tract, a contribution concerning, 286
Adnexa, the treatment of purulent, 285
Alkalinity of the blood in eclampsia, diminished, 434
Anomalies of the placenta in undeveloped uteri (hypoplasia uteri) and their results, structural, 278
Appendicitis complicating pregnancy, 195
and pregnancy, 196
Arterio-sclerosis of the uterus as a causal factor in uterine hæmorrhage, 204
Ascites in cases of myoma uteri, 349
Autothrombosis, 127

Birth in a case of uterus bicornis unicollis, triple, 33
British Gynæcological Society, 145, 348, 435
Bulletin of the Lying-in Hospital of the City of New York, 188

Cæsarean section scar, rupture of a, 282
and bilateral oöphorectomy, relapse. A case of puerperal oöphorectomy, 409
Calculus, vesical, in women, the etiology, pathogenesis and treatment of, 306
Canal, three cases of injury of the parturient, 403
Cancer of the cervix, glandular invasion in, 37
Of the cervix uteri, results of abdominal hysterectomy on, 38
Cancer of the cervix during pregnancy, vaginal hysterectomy for, 41
Abdominal hysterectomy for, 424
Of the uterus in Nurnberg, 208
Of the uterus, the value of removing the glands in the operation for, 426
Fibroids and uterine, 330
Cancer, abdominal hysterectomy for uterine, 421
Carcinoma of both Fallopian tubes, primary 53; in a woman under thirty, primary, 203
Of the vagina complicating pregnancy, primary, 198
Recti, pregnancy and, 281
Carcinoma of the uterus, involvement of the urinary tract in, 334
Of the clitoris, 349
Carcino-sarcoma uteri, 346
Care of the infant, 414
Cervix, glandular invasion in cancer of the, 37
Uteri, results of abdominal hysterectomy with removal of pelvic glands in cancer of the, 38
During pregnancy, vaginal hysterectomy for cancer of the, 41
Child, on excessive size of the newly-born, 130
Chorion-epithelioma malignum streptococcal infection, 319, 342
Clitoris, carcinoma of the, 349
Colpotomy as a means of operating upon intraligamentous cysts, 138
Confinement, eclamptic convulsions occurring fourteen days after, 325

Contagiousness of puerperal fever, Oliver Wendell Holmes and the, 369
Convulsions, occurring fourteen days after confinement, eclamptic, 325
Cornea, obstetric injuries of the, 131
Cyst, ovarian, illustrating grave complications, 54
Urethral 146
Large ligamentous, removed per vaginam, 210
Cystoma, double ovarian, and uterus affected wih melano-sarcoma, 166
Cystoscopy in women, direct, 140
Cysts, thyroid-like structures in ovarian, 52, 92
Colpotomy as a means of operating upon ligamentous, 138
Cysts and glands of the uterine, tubal and pelvic serosa, ciliated 211
Vaginal, 339
Of the ovary, tuberculosis of, 416

Death during or immediately after the termination of pregnancy or operation on the pelvic organs in women, 283
Deposits in the fimbriæ of the tubes, calcareous, 138
Dermoid, ovarian, with minute pedicle, 348
Displacements, a new plan of procedure in retro-uterine, 209

Eclampsia, clinical notes of a consecutive series of cases of, 14
Gangrene after, 31
Puerperal, 52
Remarks on the biological theories of, 191
With two special details of treatment, a case of, 399
Diminished alkalinity of the blood in, 434
Embolism, pulmonary, 348
Embryoma of the ovary, malignant, 84, 143
Endometritis, anencephalic fœtus removed from the uterus during an attack of gonorrhœal, 246
Endothelioma of uterus, 143
Of the fundus uteri, 145
Epithelioma of the vulva, 340

Fallopian tubes, primary carcinoma of both, 53, 203
Primary tubercle of the, 348
Repeated extra-uterine gestation in the same, 429
Female generative system, tuberculosis of the, 50
Generative tract, a contribution concerning adeno-myoma of the, 286
Insanity precipitated by pelvic disease in the, 344
Fibroid, necrobiotic, 348
Fibroids and pregnancy, uterine; subtotal hysterectomy, 207
And uterine cancer, 330
Fibro-myomata, on the indications for operation on, 43
Fistula, labour obstructed by a cervico-vaginal, 32
Fixation of the uterus, on the effect of ventral, on subsequent pregnancy and labour, 97
Ventral, what methods of, lead to trouble in labour and to ileus? 427
Fœtus, deformed, 53
Removed from uterus during an attack of gonorrhœal endometritis, anencephalic, 246
Fundus uteri, endothelioma of the, 145

Gangrene after eclampsia, 31
Genitalia, combined malignant tumours of the female, 428
Germ-content of the uterus and vagina during the normal puerperium, 174

Gestation, extra-uterine, 144; extra-uterine operation during sixth month of pregnancy, 347; in the same Fallopian tube, repeated extra-uterine, 429
Ectopic, which apparently ruptured twice, 346
Gigli's operation; Pseudo-achondroplasia, 128
Glands in the operation for cancer of the uterus, the value of removing, 426
Gloves in abdominal operations, on the use of, 396
Gynæcology, preventive injections of serum in operative, 141
Some fundamental problems in obstetrics and, 142

Hæmatocele, early tubal, 435
Hæmorrhage, arterio-sclerosis of the uterus as a causal factor in uterine, 204
Hernia of the pelvic floor, a new operation for, 339
Histology of the placenta, normally inserted and prematurely separated, 31
Hospital abdominal surgery, four years of, 349
Hysterectomy with removal of the pelvic glands in cancer of the cervix uteri, results of abdominal, 38
For cancer of the cervix uteri during pregnancy, vaginal, 41
For cancer of the cervix complicating pregnancy, abdominal, 424
Monster of seventh month removed by hysterectomy, 242
Intestinal obstruction after supra-vaginal, 288
On the superiority of abdominal hysterectomy over vaginal, 333
For uterine cancer, abdominal, 421
Subtotal: after histories of sixty cases, 432

Inauguration of the Tarnier Memorial, 189
Index Obstetricus, 59, 354
Induction of premature labour, 252
Infant, the care of the, 414
Infection, Chorion-epithelioma malignum with streptococcal, 319
Iniencephalus, 236
Injury of the parturient canal, 403
Insanity precipitated by pelvic disease in the female, 344
Intestine adherent to the stump of an ovarian tumour, small, 144
Introitus, abnormality of the, 249

Labium majus, teratoma of the (intestinal inclusion), 430
Labour obstructed by a cervico-vaginal fistula, 32
An unusual case of difficult, 202
Induction of premature, 252
In a case of uterus duplex separatus and vagina septa, 411
What methods of ventral fixation lead to trouble in, 427
Laceration, secondary repair of complete perineal, 323, 408
Lying-in Hospital of the City of New York, bulletin of the, 188

Malta fever in pregnancy, 326
Melanosarcoma, uterus affected with, 166
Metastasis in cancer of the stomach, uterine, 135
Methods of artificially dilating the pregnant and parturient uterus, 149
Milk, pasteurisation of, 34
The internal secretion of the ovary and placenta and their influence on the secretion of, 411
Monster of seventh month removed by hysterectomy, 242
Motility of the stomach in pregnancy and in the puerperium, a contribution to the study of, 203

Myoma uteri, ascites in cases of, 349
Myomectomy and an ovariotomy for fibroma during pregnancy, 297
Myxoma, pudendal, 393

Naso-pharynx in the newborn, infections of the, 34
Nephrectomy, two cases of pregnancy after, 194
Newborn, infections of the naso-pharynx in the, 34
North of England Obstetrical and Gynæcological Society, 349

Obstetrical Society of London, 52, 143, 346, 432
Obstetrics and gynæcology, some fundamental problems in, 142
Obstruction after supra-vaginal hysterectomy, intestinal, 288
Oliver Wendell Holmes and the contagiousness of puerperal fever, 369
Olshausen's Festschrift, 329
Omentum in a case of tubo-abdominal pregnancy, on decidual changes in the, 291
Operations, on the use of gloves in abdominal, 396
Organs, axial rotation in its relation to the pelvic generative, 25
Osteo-malacia, Cæsarean Section, and bilateral oöphorectomy: relapse, a case of puerperal, 409
Ova of different sizes, tubal twin-pregnancy with, 428
Ovary during pregnancy, the histology of the, 36
  Two cases of malignant embryoma of the, 84, 143
Oxygen gas into the abdomen in septic peritonitis, insufflation of, 213

Parotitis following injury or disease of the abdominal and pelvic viscera, 50
  Of abdominal origin, 343
Parturition, complete inversion of the uterus in, 250
Parturition, ruptures and lacerations of the vagina in connection with, 350
Pasteurisation of milk, 34
Perineum, secondary operation for complete rupture of 1, 170
Peritonitis, insufflation of oxygen gas into the abdomen in septic, 213
  Diffuse septic, 214
Placenta, histology of the, 31
  In undeveloped uteri, structural anomalies of the, 278
Pneumonia acquired in utero, 412
Pregnancy, the histology of the ovary during, 36
  Repeated tubal, 52
  Ruptured interstitial, 53
  Pyelitis in, 73
  Ulcer of the stomach and, 126
  On the relations of vessels in tubal; autothrombosis, 127
  The diagnostic importance of X-rays in, 193
  After nephrectomy, two cases of, 194
  Appendicitis complicating, 195
  Appendicitis and, 196
  Primary carcinoma of the vagina complicating, 198
  Tuberculosis of, 199
  Tuberculosis after, 325
  Study of the motility of the stomach in, 203
  Uterine fibroids and, subtotal hysterectomy, 207
  Pyelonephritis of, 221
  Toxæmia, clinical types of, 272
  Pernicious vomiting in, 275
  And carcinoma recti, 281
  Sudden death during or immediately after the termination of, 283
  On decidual changes in the omentum in a case of tubo-abdominal, 291
  A myomectomy and an ovariotomy for fibroma during, 297
  Malta fever in, 326
  Extra-uterine gestation, operation during the sixth month of, 347
Procidentia, new operation for, 339

Prophylaxis of hereditary syphilis, the, 35
Pseudo-achondroplasia: Gigli's operation, 128
Pseudo-ascites in ovarian tumours, the significance of, 204
Pubiotomy, 203, 329
Puerperium, the germ-content of the uterus and vagina during the normal, 174
Pyelitis in pregnancy, 73
Pyelonephritis of pregnancy, 221
Gravidarum et puerperarum, 327

Quadruplets, a case of, 280

Repair of complete perineal laceration, secondary, 323, 408
Resection of the tube, spontaneous, 136
Retroflexion, on, 335
Reviews of recent books:—
'A Contribution to the Pathology of the Endometrium,' Jessie M. Macgregor, 215
'A Manual of Midwifery for Students and Practitioners,' Henry Jellett, 293
'A Text-book of Obstetrics,' Adam H Wright, 351
'Essentials of Obstetrics, arranged in the form of questions and answers prepared especially for students of medicine, by W. E. Ashton.
'Gynakologische Diagnostik,' Max Henkel, 351
'Les Dégénérescences des Fibro-Myomes de l' Uterus,' G. Piquand, 217
'The Diagnosis of Diseases of Women,' Palmer Findley, 147
Rotation in its relation to the pelvic generative organs, axial, 25
Rotunda Hospital, Discussion on the Report of the, 55
Royal Academy of Medicine, Ireland, 54
Rupture of the perineum, secondary operation for complete, 1, 170
Of a Cæsarean section scar, 282

Sarcoma of the uterus, 133, 418
Secretion of the ovary and placenta and their influence on the secretion of milk, the internal, 411
Serosa, ciliated cysts and glands of the uterine, tubal and pelvic, 211
Serum in operative gynæcology, preventive injections of, 141
Sterility, indications for and methods of treatment of, 289
Struma ovarii, on, 415
Surgery of the ureter, 291
Symphysiotomy (Pubiotomy after Gigli, the indications for, 329
Syncytium and the trophoblast, the origin of the, 189
Syphilis, the prophylaxis of hereditary, 35

Tarnier Memorial, inauguration of the, 189
Temperature in anæmic patients, post-operative, 141
Teratoma of the labium majus (intestinal inclusion), 430
Thyroid-like structures in ovarian cysts, 52, 92
Toxæmia, pregnancy, clinical types of, 272
Trophoblast, the origin of the syncytium and the, 189
Tube, spontaneous resection of the, 136
Tubercle of the Fallopian tube, primary, 348
Tuberculosis of the female generative system, 50
And pregnancy, 199
After pregnancy, 325
Of cysts of the ovary, 416
Tubes, calcareous deposits in the fimbræ of the, 138
Tumour, small intestine adherent to the stump of an ovarian, 144
Tumours of the Uterus, fibroid, 44: their surgical treatment, 206
Tumours, the significance of pseudo-ascites in ovarian, 204
Of the female genitalia, combined malignant, 428

Twin-pregnancy with two ova of different sizes, tubal, 428
Repeated extra-uterine gestation in the same Fallopian tube, 429

Ulcer of the stomach and pregnancy, 126
Of the vagina and cervix, three rare forms of, 336
Ureter, surgery of the, 291
Uterus bicornis unicollis, triple birth in case of, 33
Fibroid tumours of, 44, 206
Ruptured, 54
Uterus, on the effect of ventral fixation of the, 97
On ventroscopy in rupture of the, 129
Sarcoma of the, 133, 418
Endothelium of, 143
Large fibrotic, with calcification of the arteries, 144
Methods of artificially dilating the pregnant and parturient, 149
And vagina during the normal puerperium, the germ-content of, 174
Cancer of the, in Nurnberg, 208
In parturition, complete inversion of the, 250
Uterus, involvement of the urinary tract in carcinoma of the, 334
Tuberculous disease of the, 348
Duplex separatus and vagina septa, labour in a case of, 411

Vagina, total absence of the, and its surgical treatment, 47
Two cases of chorionepithelioma of the, 139
And cervix, three rare forms of ulcer of the, 336
Rupture and lacerations of the, in connection with parturition, 350
Ventroscopy in rupture of the uterus, 129
Viscera, parotitis following injury or disease of, abdominal and pelvic, 50
Vomiting in pregnancy, pernicious, 275
Vulva, epithelioma of the, 340

Women, direct cystoscopy in, 140
Calculus in, 305

X-rays in pregnancy, diagnostic importance of, 193

INDEX OF AUTHORS.

Abbott, Maud E., and Lockhart, F. A. L., 236
Addinsell, 348
Andrews, H. Russell, 53, 97

Baisch, 426
Ballantyne, J. W., 326
Batini, E., 194
Bell, R. Hamilton, 52, 92
Biancardi, Guido, 31
Blount, Annie E., 211
Bochenski and Gröbel, 412
Boxall, 144
Briggs, H., 349
Brownlee, James, 174
Buchanan and Thomson, 131
Burdzinski, 139

Chartier and Doléris, 207
Child, Charles G., jun., 323
Clifton, Edgar J., 272
Coe, Henry C., 195
Collins, J. Rupert, and Dighton, Adair A., 250
Condamin, A. and R., 41
Couvelaire, 32, 135
Corner, E. M., 288
Costa, R., 213
Cotterill, J. M., 343
Cova, 198
Crile, G. W., 339
Cullen, Thomas S., 339
Cullingworth, C. J., 1, 53, 369, 408
Cumston, C. G., 221, 306
Cuscadon, G., 325

Davis, E. P., 283
De Jong and Semmelin, 286
Dighton, A. Adair, and Collins, J. Rupert, 250
Dittrick, Howard, 340
Doléris and Chartier, 207
Doran, Alban, 297, 432
Duclaux and Herrenschmidt, 430
Dyball, 50

Edgar, John, 170
Eisendrath, D. N., 214

Fairbairn, J. S., 144
Fehling, 285
Fellner, Otfried O., 127
Fenwick, Bedford, 348
Findley, Palmer, 204, 428
Fitzgerald, W. Gordon, 350
Flatau, 208
Fossati, G., 203
Fothergill, W. E., 403
Frank, 33
Franqué, Otto v., 329
Frédéricq, 141
Freund, R., 291

Galabin, 348
Gorgibowski, 193
Grimsdale, T. B., 396
Gröbel and Bochenski, 412
Guicciardi, 136
Gutbrod, 31

Halban, 411
Hammerschlag, 289
Hare, C. H., 203
Hartmann, Henri, 333
Hartmann and Lecène, 37
Hastings-Tweedy, E., 54
Haultain, W. J. H., 25
Henkel, 282
Henrotay, J., 141
Herrenschmidt and Ducleux, 430
Hicks, H. T., and Targett, J. B., 84, 143
Higgins, R. R., 344
Holditch-Leicester, Surg.-Capt. J. C., 52, 346
Holst, Max v., 325
Hörmann, Karl, 204

Jacobs, 38
Jacoby, 130
Jardine, Robert, 14
Jessett, F. Bowreman, 146

Koblanck, 334

Labhardt, 191

Laurens, Georges, and Pierra, Louis, 34
Lecène and Hartmann, 37
Le Play, 126
Lockhart, F. A. L., 342
Lockhart, F. A. L., and Abbott, Maud E., 236
Longridge, C. Nepean, 399, 434
Luys, 140

Macnaughton-Jones, 146, 242, 246, 249, 348, 435
Malsbary, G. E., 199
Marshall, G. Balfour, 166
Martin, F. H., 206
Maunsell, R. Charles B., 145
Michin, 429
Montgomery, E. E., 209

Nighoff, G. C., 281

Opitz, Erich, 327
Ott, V., 210

Paterson, Herbert Y., 144, 347
Penkert, M., 291
Perret, 34
Pierra, Louis, and Laurens, Georges, 34
Pinard, 35
Pinto, Carlo, 36
Piquand, G., 133, 330, 418
Pollak 411
Pollosson, Auguste, 421, 424
Pollosson and Violet, 416
Porak, 128, 203

Raw, Nathan, 319
Roberts, Lloyd, 349
Roberts, L. W., 280
Rossier, A., 409

Saniter, Robert, 428
Schaeffer, O., 278
Scharlieb, Mrs., 143
Seegert, Paul, 427
Semmelink and De Jong, 286
Sfameni, P., 139
Sheill, J. S., 202
Sianojenski, 138
Simpson, G. F. Barbour, 393
Sinclair, Sir William J., 149
Singer, Charles, 53
Sliwinski, 129
Smith, A. J., 54
Smith, G. Bellingham, 73
Spencer, Herbert R., 346

Targett, J. B., and Hicks, H. T., 84, 143
Taylor, Frank E., 348
Thomson, 50
Thomson and Buchanan, 131
Tweedy, E. A., 55

Ulesco-Stroganowa, 415

Vautrin, 47, 336
Vedeler, B. C., 335
Violet and Pollosson, 416

Wagner, 138
Webster, J. C., 44, 142, 196
Williams, J. Whitridge, 275
Williamson, Herbert, 252
Winter, 43

Vol. VIII. No. 6.] [December, 1905.

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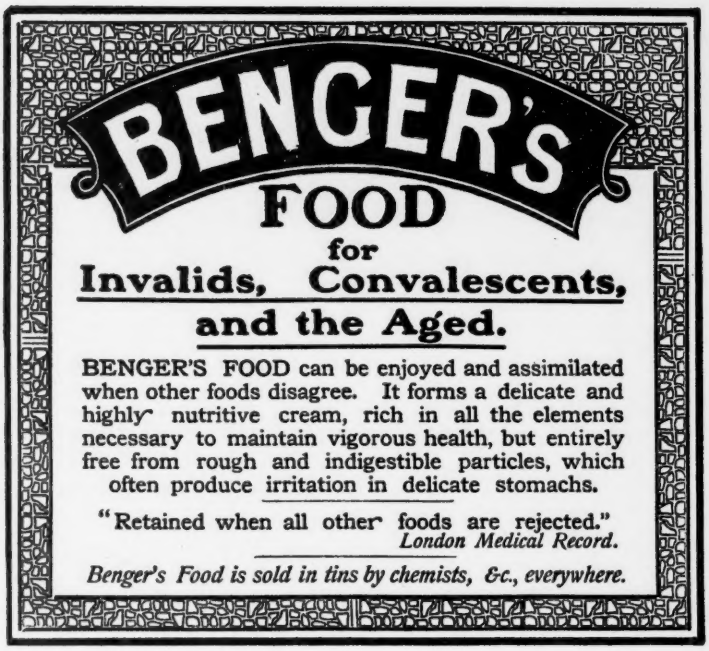
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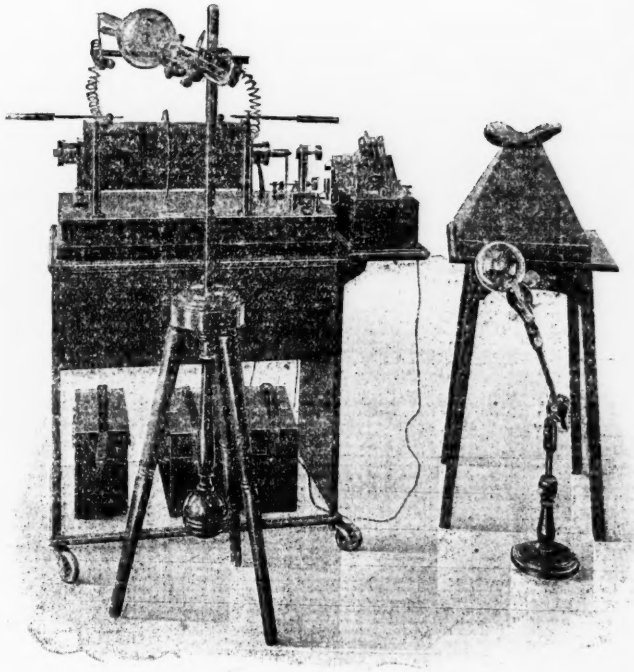

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TABLE OF CONTENTS.

ORIGINAL COMMUNICATIONS.

PAGE

1. Oliver Wendell Holmes and the Contagiousness of Puerperal Fever. By CHARLES J. CULLINGWORTH, M.D., F.R.C.P. 369
2. Pudendal Myxoma. By G. F. BARBOUR SIMPSON, M.D,, F.R.C.P.E., F.R.C.S.E., Senior Assistant to the Professor of Midwifery, University of Edinburgh; Gynæcological Tutor to the University Class of Clinical Medicine, Royal Infirmary, Edinburgh, etc. 393
3. On the Use of Gloves in Abdominal Operations. By T. B. GRIMSDALE, B.A., M.D., Hon. Gynæcologist, Liverpool Royal Infirmary; Lecturer on Gynæcology, University of Liverpool 396
4. A Case of Eclampsia with Two Special Details of Treatment. By C. NEPEAN LONGRIDGE, M.D. (Vict.), F.R.C.S. (Eng,), M.R.C.P., (Lond.), late Resident Medical Officer at Queen Charlotte's Hospital 399
5. Three Cases of Injury of the Parturient Canal. By W. E. FOTHERGILL, M.A., B.Sc., M.D., Manchester 403

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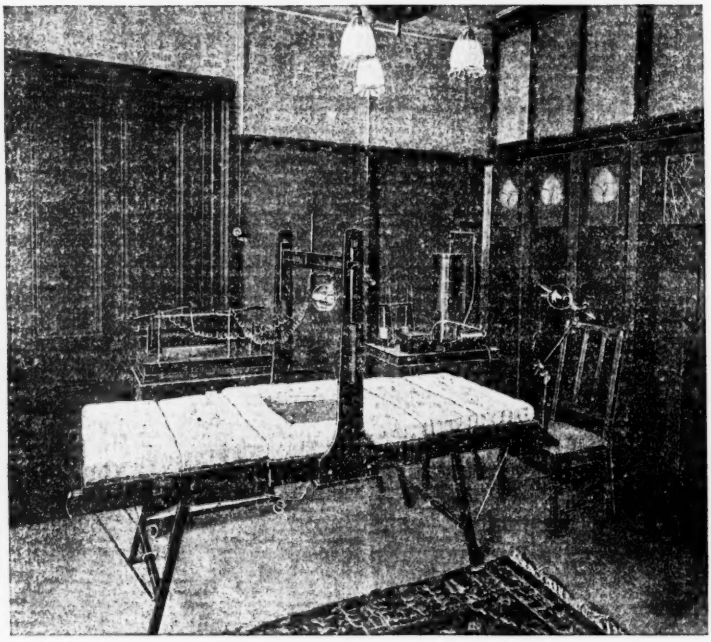

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TABLE OF CONTENTS—*continued.*

REVIEWS OF CURRENT LITERATURE.

PAGE

Obstetrics 409

Gynæcology 415

REPORTS OF SOCIETIES.

Obstetrical Society of London 432

British Gynæcological Society 435

Reviews 436

INDEX.

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